THURSDAY, SEPTEMBER 18, 1873

SCIENTIFIC WORTHIES

I.—FARADAY

Michael Faraday, born September 22, 1791, *died August* 25, 1867.

WITH this number of NATURE we present to our subscribers the first of what we hope will be a long series of Portraits of Eminent Men of Science.

This first portrait is one of Faraday, engraved on steel, by Jeens, from a photograph by Watkins. Those who had the happiness of knowing Faraday best will best appreciate the artist's skill—he has indeed surpassed himself, for the engraving is more life-like than the photograph. We could ill spare such a memorial of such a man, one in which all the beautiful simplicity of his life beams upon us. There is no posturing here!

There is no need that we should accompany the portrait with a memoir of Faraday. Bence Jones, Tyndall, and Gladstone have already lovingly told the story of the grand and simple life which has shed and will long continue to shed such lustre on English Science, and their books have carried the story home to millions; nor is there any need that we should state why we have chosen to commence our series with Faraday; everybody will acknowledge the justice of our choice.

But there is great need just now that some of the lessons to be learnt from Faraday's life should be insisted upon, and we regard it as a fortunate circumstance that we have thus the opportunity of insisting upon them while our Scientific Congress is in session, and before the echoes of the Address of the President of the British Association for the Advancement of Science have died away.

In the first place, then, we regard Faraday at once as the most useful and the most noble type of a scientific man. The nation is bigger and stronger in that Faraday has lived, and the nation would be bigger and stronger still were there more Faradays among us now. Prof. Williamson, in his admirable address, acknowledges that the present time is "momentous." In truth the question of the present condition of Science and the ways of improving it, is occupying men's minds more than it has ever done before; and it is now conceded on all sides that this is a national question, and not only so, but one of fundamental importance. Now what is the present condition of English Science? It is simply this, that while the numbers of our professors and their emoluments are increasing, while the number of students is increasing, while practical instruction is being introduced and text-books multiplied, while the number and calibre of popular lecturers and popular writers in Science is increasing, original research, the fountain-head of a nation's wealth, is decreasing.

Now a scientific man is useful as such to a nation according to the amount of new knowledge with which he endows that nation. This is the test which the nation, as a whole, applies, and Faraday's national reputation rests on it. Let the nation know then that the real difficulty at present is this; we want more Faradays; in other words more men working at new knowledge.

It is refreshing to see this want so clearly stated in the Presidential Address:

"The first thing wanted for the work of advancing science is a supply of well-qualified workers. The second thing is to place and keep them under the conditions most favourable to their efficient activity. The most suitable men must be found while still young, and trained to the work. Now I know only one really effectual way of finding the youths who are best endowed by nature for the purpose; and that is to systematise and develop the natural conditions which accidentally concur in particular cases, and enable youths to rise from the crowd.

"Investigators, once found, ought to be placed in the circumstances most favourable to their efficient activity.

"The first and most fundamental condition for this is, that their desire for the acquisition of knowledge be kept alive and fostered. They must not merely retain the hold which they have acquired on the general body of their science; they ought to strengthen and extend that hold, by acquiring a more complete and accurate knowledge of its doctrines and methods; in a word, they ought to be more thorough students than during their state of preliminary training.

"They must be able to live by their work, without diverting any of their energies to other pursuits; and they must feel security against want, in the event of illness or in their old age.

"They must be supplied with intelligent and trained assistants to aid in the conduct of their researches, and whatever buildings, apparatus, and materials may be required for conducting those researches effectively.

"The desired system must therefore provide arrangements favourable to the maintenance and development of the true student-spirit in investigators, while providing them with permanent means of subsistence, sufficient to enable them to feel secure and tranquil in working at science alone, yet not sufficient to neutralise their motives for exertion; and at the same time it must give them all external aids, in proportion to their wants and powers of making good use of them."

Whether the scheme proposed by Dr. Williamson to bring such a state of things about will have the full success he anticipates is a matter of second-rate importance; what is of importance is, that the need of some scheme is now fully recognised.

So far the remarks we have made have been suggested by Faraday's usefulness. It is to be hoped that the nobleness of his simple, undramatic life, will live as long in men's memories as the discoveries which have immortalised his name. Here was no hunger after popular applause, no jealousy of other men's work, no swerving from the well-loved, self-imposed task of "working, finishing, publishing."

"The simplicity of his heart, his candour, his ardent love of the truth, his fellow-interest in all the successes, and ingenuous admiration of all the discoveries of others, his natural modesty in regard to what he himself discovered, his noble soul—independent and bold—all these combined, gave an incomparable charm to the features of the illustrious physicist."

Such was his portrait as sketched by Dumas, a man cast in the same mould. All will recognise its truth. Can men of science find a nobler exemplar on which to fashion their own life? Nay, if it were more widely followed than it is, should we not hear less of men falling away from the "brilliant promise" of their youth, tempted by "fees," or the "applications of Science," or the advantages attendant upon a popular exposition of other men's work? Should

we not hear a little less frequently than we do that research is a sham, and that all attempts to aid it savour of jobbery?

Lastly we may consider Faraday's place in the general history of Science; this is far from easy. Our minds are still too much occupied with the memory of the outward form and expression of his scientific work to be able to compare him aright with the other great men among whom we shall have to place him.

Every great man of the first rank is unique. Each has his own office and his own place in the historic procession of the sages. That office did not exist even in the imagination, till he came to fill it, and none can succeed to his place when he has passed away. Others may gain distinction by adapting the exposition of science to the varying language of each generation of students, but their true function is not so much didactic as pædagogic—not to teach the use of phrases which enable us to persuade ourselves that we understand a science, but to bring the student into living contact with the two main sources of mental growth, the fathers of the sciences, for whose personal influence over the opening mind there is no substitute, and the material things to which their labours first gave a meaning.

Faraday is, and must always remain, the father of that enlarged science of electro-magnetism which takes in at one view, all the phenomena which former inquirers had studied separately, besides those which Faraday himself discovered by following the guidance of those convictions, which he had already obtained, of the unity of the whole science.

Before him came the discovery of most of the fundamental phenomena, the electric and magnetic attractions and repulsions, the electric current and its effects. Then came Cavendish, Coulomb, and Poisson, who by following the path pointed out by Newton, and making the forces which act between bodies the principal object of their study, founded the mathematical theories of electric and magnetic forces. Then Örsted discovered the cardinal fact of electro-magnetic force, and Ampère investigated the mathematical laws of the mechanical action between electric currents.

Thus the field of electro-magnetic Science was already very large when Faraday first entered upon his public career. It was so large that to take in at one view all its departments required a stretch of thought for which a special preparation was necessary. Accordingly, we find Faraday endeavouring in the first place to obtain, from each of the known sources of electric action, all the phenomena which any one of them was able to exhibit. Having thus established the unity of nature of all electric manifestations, his next aim was to form a conception of electrification, or electric action, which would embrace them all. For this purpose it was necessary that he should begin by getting rid of those parasitical ideas, which are so apt to cling to every scientific term, and to invest it with a luxuriant crop of connotative meanings flourishing at the expense of the meaning which the word was intended to denote. He therefore endeavoured to strip all such terms as "electric fluid," "current," and "attraction" of every meaning except that which is warranted by the phenomena themselves, and to invent new terms, such as "electrolysis," "electrode," "dielectric," which suggest no other meaning than that assigned to them by their definitions.

He thus undertook no less a task than the investigation of the facts, the ideas, and the scientific terms of electromagnetism, and the result was the remodelling of the whole according to an entirely new method.

That old and popular phrase, "electric fluid," which is now, we trust, banished for ever into the region of newspaper paragraphs, had done what it could to keep men's minds fixed upon those particular parts of bodies where the "fluid" was supposed to exist.

Faraday, on the other hand, by inventing the word "dielectric," has encouraged us to examine all that is going on in the air or other medium between the electrified bodies.

It is needless to multiply instances of this kind. The terms, field of force, lines of force, induction, &c., are sufficient to recall them. They all illustrate the general principles of the growth of science, in the particular form of which Faraday is the exponent.

We have, first, the careful observation of selected phenomena, then the examination of the received ideas, and the formation, when necessary, of new ideas; and, lastly, the invention of scientific terms adapted for the discussion of the phenomena in the light of the new ideas.

The high place which we assign to Faraday in electromagnetic science may appear to some inconsistent with the fact that electromagnetic science is an exact science, and that in some of its branches it had already assumed a mathematical form before the time of Faraday, whereas Faraday was not a professed mathematician, and in his writings we find none of those integrations of differential equations which are supposed to be of the very essence of an exact science. Open Poisson and Ampère, who went before him, or Weber and Neumann, who came after him, and you will find their pages full of symbols, not one of which Faraday would have understood. It is admitted that Faraday made some great discoveries, but if we put these aside, how can we rank his scientific method so high without disparaging the mathematics of these eminent men?

It is true that no one can essentially cultivate any exact science without understanding the mathematics of that science. But we are not to suppose that the calculations and equations which mathematicians find so useful constitute the whole of mathematics. The calculus is but a part of mathematics.

The geometry of position is an example of a mathematical science established without the aid of a single calculation. Now Faraday's lines of force occupy the same position in electromagnetic science that pencils of lines do in the geometry of position. They furnish a method of building up an exact mental image of the thing we are reasoning about. The way in which Faraday made use of his idea of lines of force in co-ordinating the phenomena of magneto-electric induction* shows him to have been in reality a mathematician of a very high order

* To estimate the *intensity* of Faraday's scientific power, we cannot do better than read the first and second series of his "Researches," and compare them, first, with the statements in Bence Jones's "Life of Faraday," which tells us the tales of the first discovery of the facts, and of the final publication of the results, and second, with the whole course of electromagnetic science since, which has added no new idea to those set forth, but has only verified the truth and scientific value of every one of them.

—one from whom the mathematicians of the future may derive valuable and fertile methods.

For the advance of the exact sciences depends upon the discovery and development of appropriate and exact ideas, by means of which we may form a mental representation of the facts, sufficiently general, on the one hand, to stand for any particular case, and sufficiently exact, on the other, to warrant the deductions we may draw from them by the application of mathematical reasoning.

From the straight line of Euclid to the lines of force of Faraday this has been the character of the ideas by which science has been advanced, and by the free use of dynamical as well as geometrical ideas we may hope for a further advance. The use of mathematical calculations is to compare the results of the application of these ideas with our measurements of the quantities concerned in our experiments. Electrical science is now in the stage in which such measurements and calculations are of the greatest importance.

We are probably ignorant even of the name of the science which will be developed out of the materials we are now collecting, when the great philosopher next after Faraday makes his appearance.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

Tyndall and Tait

I HAVE hitherto refrained from intruding upon your space with reference to this deplorable Forbes' controversy, but now that the occasion has come when a brief deliverance on my part seems called for, I trust to your courtesy, if not to your justice, to allow me room for it.

In the first place I would ask permission to inform such of your readers as may feel an interest in the subject, that if they wish to form a correct opinion of the tone and logic of my rejoinder to Principal Forbes and his biographers, they will consult the rejoinder itself, as published by Longmans, and not the extracts and inferences of Professor Tait.

They will thus learn, among other things, that what Professor Tait calls "plausible ," is simply unanswerable.

With regard to the taking up of the various points in Principal Forbes's reply, item by item, that may be done some day should I deem it a worthy occupation. In my rejoinder I converged attention on the two points which Principal Forbes himself considered the really serious ones, and having broken the neck of the argument in both these cases I cared little about prolonging the controversy. Nevertheless if circumstances show it to be necessary it may be prolonged.

Professor Tait invariably writes on the hypothesis that what is not contradicted cannot be contradicted, and must therefore be accepted as true—a natural, if not inevitable, assumption on his part. For example, Forbes's argument regarding the crevasses of Rendu was left unanswered by me, hence the conclusion that it was unanswerable. That argument, however, is now in shreds, as it might have been, had I so willed, any time during the last dozen years. Again, Principal Forbes makes an assertion regarding his tutelage of Agassiz; the assertion is left uncontradicted; it must therefore be accepted as true, and I am unjust because I do not so accept it. Thirteen years ago, however, I was in possession of a diametrically opposite assertion from M. Agassiz. Quite as distinctly, though not so specifically, he writes thus within the present year. "When Forbes came to visit me upon the glacier of the Aar, he knew not only everything that I had done, but also my plans for the future. When he left he positively declined to express any opinion concerning glacier phenomena, under the plea that he only came to gratify his curiosity, and had no intention of following up the subject, as he had no desire to be involved in the controversy then raging regarding the former extension of glaciers.* When he showed his hand I did not enter into a protracted discussion, but simply made a statement of facts and let the matter rest. . . . . When I look," adds M. Agassiz, "on the whole transaction it seems incredible. There is in it no vestige either of the gentleman or the honest investigator."

With statements of this character confronting the assertions of Principal Forbes, the proper course for me was to ignore assertions on both sides, and to confine myself to demonstrable facts. This I accordingly did.

With regard to Mr. Tait's criticism of my "popular" writings it has, of course, nothing to do with his defence of Forbes, but is the product of mere ignoble spite. He asks me to reply to him not according to the letter, but according to the spirit of his attack. If I might use the expression I would say, "God forbid!" for how could I do so without lowering myself to some extent to his level. The antecedents of Mr. Tait with reference to me are pretty well known. When I sought to raise from the dust a meritorious man whose name is now a household word in science, who has been elected by acclamation a member of the French Academy, and who has received the crowning honour of the Royal Society—when I sought to place Dr. Mayer in the position which he now holds, and from which no detraction can remove him, it was Mr. Tait who, in *Good Words*, charged me with misleading the public; who followed up his attack in the "Philosophical Magazine," and who when publicly hoisted by his own petard, retired to void his venom against me in the anonymous pages of the "North British Review." It is this man whose blunders and whose injustice have been so often reduced to nakedness, without ever once showing that he possessed the manhood to acknowledge a committed wrong, who now puts himself forward as the corrector of my errors and the definer of my scientific position. That position is happily not dependent upon him, and his opinion regarding it, is to me, as it will be to most others, a trifle light as air. But graver considerations than mere personal ones here arise. Might I venture, Mr. Editor, to express a doubt as to the wisdom of permitting discussions of this kind to appear in your invaluable journal. Having opened your columns to attack you are, of course, in duty bound to open them to reply, but if I might venture a suggestion, you would wisely use your undoubted editorial rights, andconsult the interests of science, by putting a stop to proceedings which dishonour it. An illustrious person writes to me thus :—"I have just read Professor Tait's letters in NATURE, and feel a recurrence of that pain which similar communications once inflicted on myself—pain felt, not on my own account, for I knew that the attacks would no more sully me in the opinion of those whom I loved and respected, than they did in my own opinion; but pain for the wounded honour of science and the outraged dignity of scientific controversy."

JOHN TYNDALL

Athenæum Club, Sept. 16

[We deeply sympathise with Professor Tyndall's remarks on the injury done to scientific controversy by the introduction into it of personalities, and we should have made his own letter square with his canon if his reference to our duty in this matter, and his insinuation of injustice did not take the matter out of our hands. Prof. Tyndall forgets (1) that Prof. Tait's letter is an answer to a pamphlet by Dr. Tyndall, and that space was asked for it as such; and not an *attack* in the sense in which Prof. Tyndall uses the word; (2), that if the Editor were to assume the power and responsibility that Prof. Tyndall suggests, NATURE might easily fall from the position of absolute justice and impartiality in all scientific matters which it now occupies and become the mere mouthpiece of a clique.

What the Editor can do and has endeavoured to do in this case, is to guard the reputations of men of Science against the attacks of men of straw, and to see that no personalities are used; and it is under strong protest that he allows to pass in Prof. Tyndall's letter, for the reasons already stated, personalities, the equivalents of which, the Editor, in the exercise of his "undoubted editorial rights," struck out of Prof. Tait's communication.—ED. NATURE.]

* This tallies with Forbes's own account (Travels, page 38). "Far from being ready to admit, as my sanguine companions wished me to do in 1841, that the theory of glaciers was complete, and the cause of their motion certain, after patiently hearing all they had to say, and reserving my opinion, &c." This reservation of opinion is probably the reticence referred to by Agassiz.

NOTES FROM THE "CHALLENGER"
VII.

ON Monday the 30th of June we sounded in 1,000 fathoms, about 114 miles westward from Fayal. The dredge was put over early in the forenoon, and came up half filled with a grey sandy ooze with a large proportion of the dead shells of Pteropods, many Foraminifera, and many pebbles of pumice. Many animal forms of great interest were found entangled in the swabs, or sifted out of the mud. Another Schizopod crustacean of large size and great beauty of form and brilliancy of colouring came up in this haul. Dr. von Willemoes-Suhm regards it as congeneric with the species taken at Station 69, at a

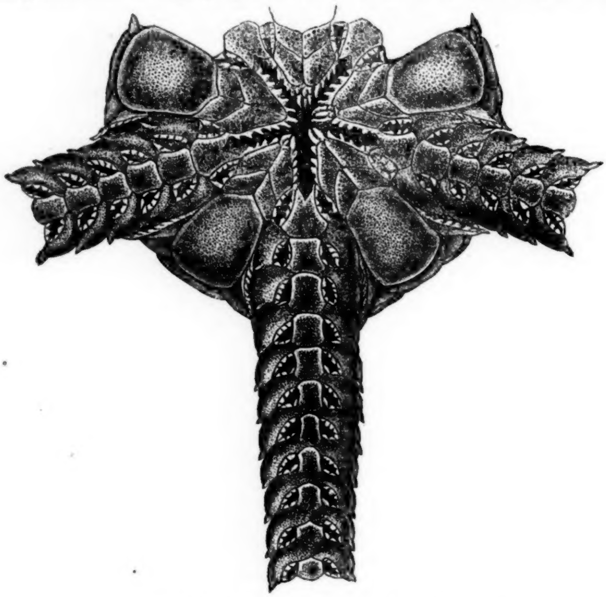

FIG. 1.—Ophioglypha bullata, Wy. Thomson—six times the natural size.

depth of 2,200 fathoms, and as these crustaceans are among our most interesting acquisitions during the voyage between Bermudas and the Açores, I will abstract a brief description of them from his notes.

The two crustaceans for whose reception Dr. von Willemoes-Suhm proposes to establish the genus *Gnathophansia* present characters which have hitherto been found partly in Schizopods and partly in Phyllopods, but not combined in the same animal. They are, however, essentially Schizopods, and have much in common with

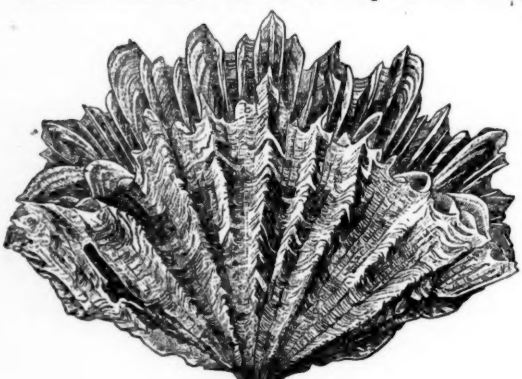

FIG. 2

FIG. 2.—Flabellum alabastrum, H. N. M.

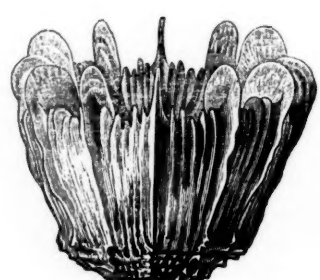

FIG. 3

FIG. 3.—Ceratotrochus nobilis, H. N. M.

Lophogaster, a genus described in great detail by the late Prof. Sars. It is proposed to refer *Gnathophansia* to the family Lophogastridæ, which must be somewhat modified and expanded for its reception.

In *Gnathophansia* the dorsal shield covers the thoracic segments of the body, but it is unconnected with the last five of these. The shield is prolonged anteriorly into a spiny rostrum. The stalked eyes are fairly developed in the ordinary position. There is an auxiliary eye on each of the maxillæ of the second pair.

The two species of the genus are thus distinguished: *G. gigas*, n. sp. (Figs. 4 and 5). Scale of the outer an-

tenna with five teeth; dorsal shield with the outer angles of its posterior border produced into spines; no posterior spine in the middle line; length 142 mm. Of this species one specimen was taken from a depth of 2,200 fathoms, with a bottom of Globigerina ooze, at Station 69, 400 miles to the west of the Açores.

G. zoëa, n. sp. (Fig. 6): Scale of the outer antenna with one tooth. A long central spine on the posterior border of the dorsal shield, but no lateral spines; length, 60 mm. A single specimen at the present station likewise from a bottom of Globigerina ooze.

On comparing the figures of these two species and of their anatomical details with that of *Lophogaster* given by Sars, one is struck by their great general similarity; but

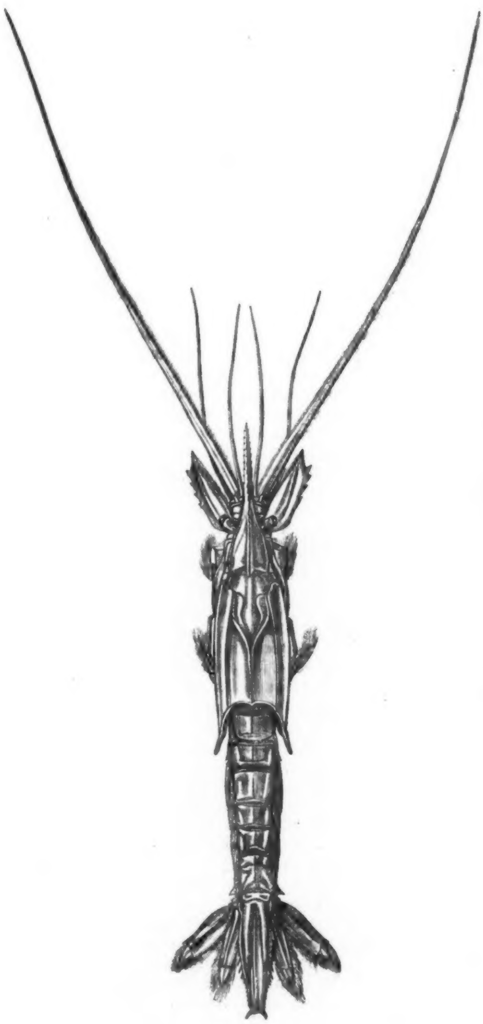

Fig 4

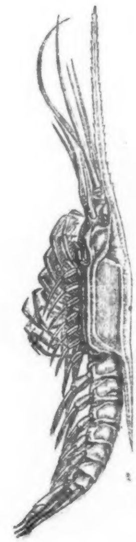

Fig 6

Fig. 5

Figs. 4 & 5.—Gnathopha sia gigas, v. W.-S.

Fig. 6.—Gnathophansia zoea, v. W.-S.

there are characters presented by the new genus, particularly in connection with the dorsal shield, which not only entirely separate it from Lophogaster, but enlarge our views on the whole Schizopod group. In both species the shield is sculptured by ridges traversing it in different directions, and in both there is a long spiny rostrum; but this shield is merely a soft duplicature of the skin, connected with the body only anteriorly, and leaving five thoracic segments entirely free. In the structure of the shield and its mode of attachment *Gnathophansia* has the greatest resemblance to *Apus* among all crustaceans, but it differs from it widely in all other respects. *Nebalia* is the only Schizopod in which the carapace is not connected with the posterior thoracic segments, but in that genus the form of the carapace is totally different, and the genera are otherwise in no way nearly related.

Neither the antennæ, nor the scales, nor the parts of the mouth present any marked differences from those of

Lophogaster, with the exception of the second maxillæ. These, with nearly the same form as in the Norwegian genus, bear a pair of accessory eyes. Such eyes are well known at the base of the thoracic and even of the abdominal limbs in the Euphansidæ, a family with which the Lophogastridæ have otherwise nothing in common, but hitherto they have not been met with in any other animal or in any of the manducatory organs.

Of the eight pairs of legs seven are ambulatory, only the first pair is, as in *Lophogaster*, transformed into maxillipeds. The gills are arborescent and attached to the bases of the legs. The abdomen and its appendages scarcely differ from those of *Lophogaster*. We find here also that the last segment is apparently divided into two. This would indicate an approach to such forms as *Nebalia*, which has nine abdominal segments, or at all events a tendency to a multiplication of segments which if really existing would scarcely allow the association of the genus with the true Schizopods.

The weather was remarkably fine. During the day the island of Flores was visible like a cloud on the horizon, about 50 miles to the northward. In the afternoon we obtained a series of temperature soundings at intervals of 100 fathoms down to 1,000, and in the evening proceeded under steam towards Fayal.

On the following day, the 1st of July, we sounded in 1,350 fathoms, about 20 miles west of Fayal, apparently in a depression which separates the western group of the Açores, Flores and Corvo from the central group Fayal, Pico, San Jorge, Terceira, and Gracioza, and during the afternoon we gradually approached the fine island of Fayal, and enjoyed the development of its bold outlines and rich and varied colouring. In the evening we passed into the narrow channel between Fayal and Pico, and anchored in the roadsteads of Hortes. We found to our great disappointment that small-pox was prevalent in Fayal, and as Captain Nares considered it imprudent to give general leave, one or two of us only landed to pick up what general impression we might of the appearance of the place, and on the following morning we proceeded towards San Miguel, first taking a few hauls of the dredge in shallow water between Fayal and Pico, where we found a rather scanty fauna, resembling in character that of southern Europe, on a bottom of dark volcanic sand.

On Friday, July 4, we sounded in 750 fathoms on a rocky bottom. The ship water-bottle was sent down and brought up a sample of the bottom water. In the afternoon we shortened and furled sails, and proceeded under steam towards San Miguel, and in the evening stopped abreast of Ponta Delgada, the capital of the island, where we lay-to for the night, secured to a buoy. Next morning, as we found, greatly to our satisfaction, that the town was considered free from any epidemic of small-pox, we steamed in to the anchorage, and cast anchor in 13 fathoms.

We remained at San Miguel until Wednesday the 9th. We were well aware that the time at our disposal was quite insufficient to enable us to do anything of importance to add to the knowledge of the natural history of the island already so well worked out, and as we had had a long sea-cruise, we were in no way disinclined for a few days of complete relaxation. We accordingly combined into a large party, totally unscientific in its object, and by the aid of mules and donkeys made a most enjoyable raid among the caldeiras and volcanic ranges of the east end of the island. The random impressions collected during these *horæ subsecivæ* may perhaps be chronicled elsewhere.

Our first haul after leaving Ponta Delgada, was in 1,000 fathoms, mid-way between the islands of San Miguel and Santa Maria, and about fifteen miles north-west of the Formigas. The bottom was Globigerina ooze. The principal feature in this dredging was the unusual abundance of stony corals of the deep-sea group.

Two living specimens of a large species of *Flabellum* were sifted out, the same as the one which we had dredged previously at station 73, to the west of Fayal. The corallum is wedge-shaped, the calicle rising from an attenuated pedicle. The extreme height, from the end of the pedicle to the margin of the cup, is 50 mm.; the greatest diameter of this calicle is 65 mm., and the smallest 30 mm. The three species are very nearly of the same dimensions.

The lateral costæ make an angle with one another of 120° to 140°, and are sharp and moderately prominent, with an irregular edge. The external surface of the calicle is covered with a glistering epitheca, and near the margin is of a light pink colour. The costæ of the faces corresponding to the primary and secondary septa are almost as well marked as the lateral costæ, and appear as irregularly dental ridges, separated by slight depressions. The ends of the calicle are broadly rounded, and it is compressed laterally in the centre. The upper margin is curved, describing about one-third of a circle.

There are six systems of septa disposed in five cycles. The septa are extremely thin and fragile. They are tinged with pink, and covered with rounded granules, disposed in rows. The primary septa are approximately equal to the secondary, giving somewhat the appearance of twelve systems. These septa are broad and prominent, with a rounded superior margin, and curved lines of growth. The septa of the third, fourth, and fifth cycles successively, diminish in breadth, and are thus very markedly distinguished from one another, and from the primary and secondary septa. The septa of the fourth cycle join those of the third a short distance before reaching the columella. The septa of the fifth cycles are incomplete. The margin of the calicle is very deeply indented, the costal corresponding to the primary and secondary septa being prolonged in conjunction with the outer margins of these septa, into prominent pointed processes; similar but shorter prolongations accompany the tertiary, and some of the quaternary septa. Between each of the sharp projections thus formed, the edge of the wall of the calicle presents a curved indentation.

Two of the specimens procured, expanded their soft parts when placed in sea-water. The inner margin of the disc round the elongated oral aperture, presents a regular series of dentations, corresponding with the septa, and is of a dark madder colour; the remainder of the disc is pale pink. The tentacles take origin directly from the septa. They are elongated and conical. Those of the primary and secondary septa are equal in dimensions, and along with the tertiary tentacles, which are somewhat shorter, but in the same line, are placed nearest the mouth, and at an equal distance from it. The tentacles of the fourth and fifth cycles are successively smaller and at successively greater distances from the mouth. Placed on either side of each tentacle of the fifth cycle, and again somewhat nearer the edge of the calicle, there are a pair of very small tentacles which have no septa developed in correspondence with them. There are thus four successive rows of tentacles, and the normal number is ninety-six. The tentacles are of a light red colour, and between their bases are stripes of yellowish red and light grey.

This group belongs to the group *Flabella sub-pedicellata* of Milne-Edwards, and probably to that division in which the costæ are prominent and ridge like on the faces of the corallum, as well as on its lateral margins, but it differs from those described under this head by Milne-Edwards, in that it has five cycles, the fifth being incomplete, and in other particulars which appear from the description given.

A single living specimen of a coral referred by Mr. Moseley to the genus *Ceratotrochus* was obtained from this haul. The corallum is white. The base sub-pedicellate with a

small scar of original adherence. The principal costals are prominent, and round the region of the base beset with small spines directed somewhat upwards. The upper portion of the costa is without spines. The primary and secondary septa are broad and exsert. Pali are absent, the columella is fascicular. The absence of pali, the form of the columella, and the nature of the base, associate this form with the *Ceratrotrochi*, as defined by Milne-Edwards.

The animal is of a dark madder colour on the region of the margin of the calicle between the exsert primary and secondary septa, and on the membrane investing the wall of the corallum from the margin down to the commencement of the spines. This dark colour is succeeded on the disc by a band of pale bluish, within which there is again a zone of very dark madder colour round the mouth. The dark colouring-matter is interesting, as it gives an absorption spectrum of three distinct bands.

On Friday, July 11, we sounded in 2,025 fathoms, 376 miles to the west of Madeira, the bottom very well marked "globigerina ooze," and the bottom temperature 1° 5′ C.

The weather for the last few days had been remarkably fine, with a pleasant light breeze. When we turned up on deck on the morning of the 16th, we were already at anchor in the beautiful bay of Funchal, and looking at the lovely garden-like island, full of anticipations of a week's ramble among the peaks and "currals" and the summer "quintas" of our friends—anticipations which were doomed to be disappointed.

WYVILLE THOMSON

THE INTERNATIONAL METRIC COMMISSION AT PARIS

IN continuation of the notices of the proceedings of this Scientific Commission (see NATURE, vol. vii. p. 237), it may now be stated that the French Section have been engaged during the present year in the work of the Commission entrusted to them, and have continued their sittings up to the present time. It appears from the printed "Procès Verbaux" that their attention has been principally directed to the further investigations and experiments required for the melting and casting of the large mass of alloy of platinum and iridium, determined upon as the material of all the new standards, with the view of obtaining a homogeneous ingot of these two metals in the proper proportions. This preliminary work is now so far completed that the twelve members of the Commission elected as the Permanent Committee, have been summoned to meet at Paris on October 1, to consult upon the subject with the French Section, and more particularly to discuss and decide the following points:—

1. The date of the definitive of the melting platinum-iridium intended for the construction of the new International metric standards.
2. The question whether the *Mètres-à-bouts* requested by some countries shall be constructed from the metal of the same melting as the *Mètres-à-traits*.
3. Whether the kilograms shall be made from the metal of the same melting as the *Mètres-à-traits*.

As to the number of metric standards required to be constructed by the Commission, the greater number of the Governments represented at the Commission have already intimated their wishes to have in all 31 metres and 24 kilograms. Germany and Italy have not yet notified their decision. Austria and Switzerland have declined to reply until the question of the creation of an International Bureau is satisfactorily settled, and it is understood that the same course is being followed by Germany. Russia is favourable to the creation of the Bureau, but has not yet decided on the number of standards she will require.

In addition to the number of fifty delegates already appointed by twenty-nine Governments to take part in the International Metric Commission, and whose names have been already announced, the Haytian Government has nominated M. Ch. Laforestie, Chargé d'Affaires of the Haytian Republic, and the Government of Brazil has nominated Prof. Such de Capanema as their respective delegates of the Commission. The French Government has also invited the Governments of Central America, Persia, China, and Japan to send delegates to take part in the proceedings of the Commission.

As it will be expedient to construct a number of spare copies of the new metric standards, it will probably be necessary to prepare for the construction of not less than fifty metres and nearly as many kilograms.

But difficulties must inevitably and at once arise at Paris from the course taken by the Governments of Germany, Austria, and Switzerland, as it tends materially to impede the attainment of the declared primary objects of the Commission to construct and furnish every Government interested with uniform metric standards, which are to be accurately verified, and of equal authority. After the expiration of four years from the date of the appointment of the Commission by the French Government, on September 2, 1869, and the passing of almost unanimous resolutions at a full meeting of the Commission in 1872, upon the mode of constructing the new standards, the time has now arrived when everything has been got ready for commencing the actual construction of the new standards. It can hardly be expected that this, the real work of the Commission, is to be stopped until the ulterior question of the creation of an International Metric Bureau is settled to the satisfaction of the three above-mentioned Governments. Nor does a further significant step which has been recently taken by the Austrian Government lead to much hope of a satisfactory solution of this question.

The Austrian Government has officially declared that it accepts in principle the establishment of an International Metric Bureau upon the basis of the resolutions passed by the Commission, so far as relates to the objects and functions of this Bureau; and that it is quite disposed to take part in a Convention upon the subject, provided that all the other Governments represented at the Commission give their adherence. But it expressly reserves the right of making new propositions when the questions of the organisation, the seat, and the direction of the Bureau are discussed, as well as the right of definitively approving the Convention.

It proposes, at the same time, that in order to maintain the international character of the negociation, the seat of the Conference shall be at Berne, where the International Telegraphic Conference is now held, or at Brussels, these two cities being equally upon neutral territory.

And that for facilitating the proceedings of the Conference, the Permanent Committee appointed by the Metric Commission, shall previously elaborate a project of Convention to be communicated to the several governments interested; and that the Conference be not convoked for completing the definitive Convention until the preliminary negociations shall be sufficiently advanced to allow of a favourable result.

The invitation given by the French Government to the Austrian and other governments, was to take part in the creation of the International Metric Bureau based upon the five points proposed by the Commission, and it now appears that Austria objects to three out of these five points. And even as regards the other two points, Austria's adhesion is conditional upon the concurrence of all the other governments represented at the Commission. Up to the present time, however, the governments of five countries only have officially notified their concurrence, whilst those of twelve countries have formally declined to take any part in the establishment of the proposed International Metric Bureau. Under these circumstances, its creation at all seems very problematical, however desirable it may be in the interests of metrological science.

It is evident that the decision upon these new propositions must be left entirely to the governments interested. At any rate, the discussion of the Austrian propositions appear to be quite beyond the powers of either the French Section or the Permanent Committee, who are in no way authorised to re-open questions which, so far as the action of the Commission is concerned, have already been unanimously decided at the full meeting of the Commission. Meanwhile, the specific work of the Commission must be proceeded with, and the approaching meeting at Paris will enable the final decisions to be made, which alone are now required for beginning the construction of the new Standards.

H. W. CHISHOLM

NOTES

AN election will be held on Thursday, October 30, to two fellowships in connection with Merton College, Oxford. The examination for one of these fellowships will be in mathematics, for the other in physical science. The election to the physical science fellowship will be decided with respect to proficiency in physics, but candidates will have an opportunity of showing a knowledge of chemistry as supplementary to physics. The examination in both these subjects will be partly practical, partly by papers, and will be held in common with Magdalen College. A lectureship in physics, tenable for three years, in Trinity College, of 200*l*. per annum, will be offered to the Fellow to be elected. The examination for the two fellowships will commence on Tuesday, October 7, at 9 A.M., in the Merton College Hall. Candidates are required to call on the Warden on Tuesday, October 7, between 4 and 5 P.M.

THE Opening Address of this session of the St. Thomas Charterhouse Teachers' Science Classes will be delivered by Mr. F. C. Buckmaster on Saturday morning, the 20th inst., at 10.30. The chair will be taken by Sir J. Bennett, and a deputation from the Science Department of South Kensington will attend. Last year this undertaking met with signal success: above 200 teachers of primary schools availed themselves of the privileges offered by the institution. Many of the late students are now qualified to give instruction in elementary science. The movement is likely to do an immense amount of good in the way of making the teaching of elementary science common amongst the masses. During the recess about 250*l*. has been expended in fitting up a chemical laboratory and purchasing scientific apparatus; this, together with the engagement of an additional number of lecturers, it is thought will again secure a large number students.

WE understand that the bryological books and exceedingly rich and important collections and preparations of mosses left by the late Prof. Sullivant, whose death we recorded last week, are consigned to the Grey Herbarium of Harvard University, with a view to their preservation and long continued usefulness. The remainder of his botanical library, his choice microscopes, and other collections are bequeathed to the State Scientific and Agricultural College just established at Columbus.

THE *American Naturalist* for August records the death of four contributors to that journal, all more or less known as working naturalists:—Prof. John Lewis Russell, of Salem, one of the founders, and for many years president of the Essex County (Massachusetts) Natural History Society, which afterwards became part of the Essex Institute, an active worker in botany; Mr. George Gibbs, of New Haven, the distinguished American ethnologist and philologist, whose special work had been in the language and history of the North American Indians; Col. John W. Foster, president of the Chicago Academy of Science, a constant contributor of papers and memoirs on geological and archæological subjects, and joint author with Prof. Whitney of the Government Report on the Mineral Lands of Lake Superior, published in 1850; and Prof. Henry James Clark, of Amherst, one of the most thorough histologists and best microscopists in the country, and a large contributor to Prof. Agassiz's volumes on the Natural History of the United States. Of these losses to science, Prof. Clark was under 50, and only Prof. Lewis over 60.

THE first meeting of the Agassiz Natural History Club, recently organised by the students of the Anderson School of Natural History on Penikese Island, was held on July 24, and showed signs of great energy and activity. Although the school had only been open a fortnight, lectures on surface geology, the embryology of vertebrates and articulates, on physiology, physical geography, on the microscope and its construction, with practical lessons on its use; free hand drawing on the blackboard, zoological and landscape drawing, and daily dredging excursions in the yacht *Sprite*, together with instructions in collecting and preserving animals, have been given. The amount of laboratory work done is stated to be most satisfactory. Large aquaria are being set up in the temporary laboratory.

THE Council of the Pharmaceutical Society are desirous of forming a complete herbarium of medical plants from every quarter of the globe, whether officinal or not. Mr. Holmes, the Curator of the Society's Museum, 17, Bloomsbury Square, will be glad to enter into communication with any foreign botanists and pharmaceutists willing to co-operate in the work.

IN a telegram from St. Petersburg, September 11, it is stated that General Kaufmann reports that the Amoo Daria river is not navigable by steamboats. The scientific expedition sent out by General Kaufmann to explore the old bed of the Amoo Daria river as far as the lake of Lara Kamish, returned on July 23 to the camp at Kunurgentsch. The expedition explored the river to a distance of 450 versts, and succeeded in collecting much valuable information and scientific materials.

IN a telegram from St. John's, Newfoundland, of September 11, it is stated that the *Juniata* had arrived there and reported that the camp of the crew of the *Polaris* was discovered by the *Tigress* on August 14 at Littleton Island, where the ship was deserted. Manuscript records of the expedition up to a period of six weeks before the discovery were secured. The *Tigress* is still in search of the Buddington party, who are believed to be safe.

A PAPER in Petermann's *Mittheilungen* upon the driftwood found in Nova Zembla has at present a special interest in connection with the discovery of fragments of a similar character by the crew of the *Polaris* in Polaris and Newman Bays. The Nova Zembla specimens consisted mainly of willow of various thicknesses. There were also, however, pieces of beech nearly a foot in diameter, several species of pine, among these *P. sylvestris*, an *Abies*, &c. It is thought that a large portion of this material must have been derived from the Petschora, Ob, and Yenesei rivers, and that none of it could have been derived from the current of the Gulf Stream.

THE past winter was very mild in the southern portion of Iceland, but quite severe in the northern. In the middle of January an eruption of the volcanoes in the great Yokul Mountains, in the south-east corner of the island, took place, which continued with unusual violence for about a week, and then suddenly ceased. Since then no fire has been noticed. Large quantities of ashes have fallen on different localities, but it is believed that the deep bed of snow protected the pasture lands from destruction. Volcanic eruptions took place at the same time in Chili.

THE recent number of *Petermann's Mittheilungen* contains articles and maps on the American North Polar Expedition and Transcaspian Russia. The New Lybian Expedition and the Russian March on Khiva are the subjects of two of the articles.

BY the death of the last surviving porpoise the Brighton Aquarium has to lament the loss of one of its most attractive features.

WE have received the Prospectus of a new club to be called "The Scientific Societies Club." The approaching concentration of scientific societies, the Prospectus says, suggests that the present is a fitting time for the formation of a "Scientific Societies Club," which would afford in the neighbourhood of Burlington House conversation and reading rooms, as well as the usual facilities of a club for members of all scientific societies. In order to render the club generally available and as useful as possible to the scientific world, it is proposed that the entrance fee and the annual subscription shall each be small.

ACCORDING to Dr. Fritsch, the discovery has lately been made of lacustrine dwellings in the vicinity of Leipsic, as the result of certain engineering operations undertaken to regulate the course of the River Elster. After passing through a series of layers at a certain depth, the workmen found a series of oak piles pointed below and decomposed above, and supporting a certain number of oak trunks placed horizontally; and on the same level with these were found certain lower jaws and teeth of oxen, fragments of antlers, broken bones of various mammifers, shells of an Anodon, fragments of pottery, two polished stone hatchets, &c.

PROF. C. A. WHITE, of Iowa State University, and State geologist of Iowa, has been appointed to the new chair of Geology and Natural History at Bowdoin College.

A COMMUNICATION has been made to the Academia dei Lincei of Rome, by M. Tarry, giving the results of his personal experience and investigations into the connection between the cyclonic storms and the showers of sand that frequently visit Southern Europe. M. Tarry, after travelling as secretary to the French Meteorological Society into Northern Africa and the Desert of Sahara, and having consulted the files of the *Daily Weather Bulletin* of the Paris Observatory, believes himself to have established the fact that whenever a cyclone passes southward from Europe over the Mediterranean Sea into Africa (as some few of them do every season), it then returns northward or northwestward, and transports the sand which in the desert formed a sand-storm to the southern coasts of Europe as a sand-shower of greater or less duration. The satisfactory investigation of this subject is much impeded by the absence of barometric observations on the southern shores of the Mediterranean; and to remedy this defect, M. Tarry has recently established new meteorological statistics at Mogadore, Morocco, Terceira, Madeira, and even in the interior of the Sahara.

"GENERAL Remarks on the Climate of Bombay, with a brief description of the Peculiarities of the Weather of the year 1871," is the title of a pamphlet which we have just received, written by Mr. Charles Chambers, F.R.S., Superintendent of the Kolaba Observatory.

THE *Times of India* states that education is making rapid progress in Ceylon, and vernacular schools will soon be within the reach of every section of the native community. The same paper states that Ceylon will contribute a selection of colonial products to the next Exhibition at South Kensington.

THE Rev. Thos. Garnier, Dean of Winchester, who died recently at the age of 98, was the "father" of the Linnean Society, having been elected during the last century, in 1798, only ten years after the foundation of the Society. Some of his contributions to botanical literature bore the date of last century.

THE additions to the Zoological Society's Gardens during the past week include a Garnet's Galago (*Galago garnetti*) from East Africa, presented by Capt. Geo. Butchart; a Manx Shearwater (*Procellaria puffinus*), British, presented by Dr. Bree; a Reeve's Muntjac (*Cervulus reevesi*), from China, presented by Mr. R. Swinhoe; a Spotted Cavy (*Cœlogenys paca*), from South America, presented by Mr. J. de Castro; three Common Chamæleons (*Chamæleon vulgaris*), from Africa, presented by Mr. W. C. Hotham; an Alligator (*Alligator sp.*), presented by Mr. W. Gillespie.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Sept. 8.—M. Bertrand in the chair.—The following papers were read:—Fifth note on Guano, by M. Chevreul.—Note on the observations of M. Lecoq de Boisbaudran, relative to the appearance of Phylloxera in the vineyards of the Charente, by M. Milne-Edwards.—Note on the number of points of intersection which represent a multiple point common to two plane curves, &c., by M. de la Gournerie.—Researches on Crystalline Dissociation, continuation by MM. P. A. Favre and C. A. Valson. This portion of the paper dealt with the valuation and division of the work done in saline solutions.—Note on a New System of representing the continuous Meteorological Observations, made at the National Observatory, Algiers, by M. Bulard.—Note on Magnetism, third part, by M. J. M. Gaugain.—On the Spontaneous Motion of Ascension of Liquids in Capillary Tubes, by C. Decharme. This portion of the paper treated of the subject from a theoretical point of view.—On Pyrogallol in the presence of iron salts, by M. E. Jacquemin.—Researches on the Spectra of Chlorophyll, by M. J. Chautard. The author has found that this substance so easily changed as viewed from the physiological point of view, is very stable when subjected to chemical reagents.—On the state of the Volcano of Nisiros, in March, 1873, by M. H. Gorceix.—M. de Laval sent a note stating that he was the original proposer of the use of the carbonic disulphide against the Phylloxera.—The ephemerides of Brorsen's Comet were received from Mr. Plummer, and a note on the same comet, and on that of Faye, from M. Stephan.—New observations on the presence of Magnesium on the Solar Limb, and an answer to certain points in M. Faye's theory, by Father Tacchini. The author stated in his letter that the fact of the line 1474 K always appearing with *b*, and even without it, induces him to think that the former is not due to iron which is much heavier than magnesium.—On the use of Chronometers at sea, by M. Magnac.—Reflections on Spontaneous generation, in relation to a note by M. Gayon, on the spontaneous changes of eggs, and a note of Mr. Grace Calvert on the power of preventing the development of Protoplasmic life, by M. A. Béchamp.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE forty-third meeting of the Association was opened yesterday evening in Bradford, when Dr. Carpenter resigned the Presidency, and was succeeded by Prof. A. W. Williamson, who delivered the opening address in St. George's Hall.

Notwithstanding that Bradford is considerably larger than Brighton, its resources in the way of sleeping accommodation have been considerably tried by the unusually large influx of visitors caused by the meeting of the Association. All the hotels, we believe, are full, as well as most of the private houses on the lists of the secretaries. Arrangements have, however, been made with the railway companies for conveying members to and from neighbouring towns where hotel accommodation may be obtained. The local secretaries, Dr. Campbell, Mr. Goddard, and Mr. Piele Thompson, have spared no pains to make the

arrangements for the reception of the members of the Association perfect; and if the meeting is not in all respects a complete success, it will be no fault of theirs, nor of the local authorities, who seem anxious to do all in their power for the comfort and enjoyment of the visitors.

A very fine town-hall was opened in Bradford a few days ago, but so far as we can learn, none of the meetings of the Association will be held in it. Ample accommodation has been provided in other buildings for the various meetings. The Sections met to-day at 11 A.M., and continue to do so till Tuesday next. Section A meets in the School Room, Horton Lane Chapel; Section B in the School Room, Unitarian Chapel; Section C in the Lecture Hall, Horton Lane Chapel; Section D in the Church Institute; Section E in the Mechanics' Institute; Section F in the West Riding Court House; and Section G in the Church Institute. To-night a *soirée* will be held in St. George's Hall: in the same place, to-morrow night, at 8.30, Professor W. C. Williamson, F.R.S., of Manchester, delivers a discourse on "Coal and Coal Plants;" on Saturday evening, at 7.30, Dr. Siemens gives a lecture to the operative classes on "Fuel;" and on Monday evening, at 8.30, Professor Clerk-Maxwell, a discourse on "Molecules." On Tuesday next, a *soirée* takes place at 8.30 P.M. in the Mechanics' Institute, where, on Wednesday, the concluding General Meeting will take place at 2.30 P.M.; on the same evening, a Grand Complimentary Concert will be given in St. George's Hall, at 8 o'clock.

A number of Reports, both those involving and those not involving grants of money, will be given in, and will no doubt be listened to with great interest by the scientific men present. We hope that this year the Association will rise to the occasion in the matter of liberality, and give a practical example of what ought to be done in the endowment of scientific research. By the courtesy of the officers we are enabled to give the Inaugural and some of the Sectional Addresses. To the same source we are indebted for the following list of some of the papers to be read in the various sections:—

SECTION A.—Lord Rayleigh: A short paper on a Natural Limit to the Sharpness of the Spectral Lines.—W. Davis: Some Abnormal Effects of Binocular Vision.—H. Muirhead: On Regelation.—G. M. Whipple: A new Electrical Anemograph; a new form of Rutherford's Minimum Thermometer; on the Passage of Squalls across the British Isles.—W. R. Birt: On the Importance and Necessity of continued Systematic Observation of the Moon's Surface.—G. O. Hanlon: Some Suggestions towards the formation of extended Tables of Logarithms.—M. Hermite: On the Irrationality of the Base of Hyperbolic Logarithms.—R. S. Ball: Dynamometers for the Measurement of Force in absolute units; A quiescent rigid body possessing three degrees of freedom receives an impulse: determine the instantaneous screw about which the body commences to twist.

SECTION B.—Messrs. A. Vernon Harcourt and F. W. Fish: On a continuous process for purifying Coal Gas from Sulphuretted Hyd. and Ammonia, and for extracting Sulphur and Ammoniacal Salts.—W. H. Pike: On several Homologues of Oxalic Acid.—Dr. Gladstone: Black Deposits of Metals.—C. Horner: On the Spectra of certain Boric and Phosphoric Acid blow-pipe beads.—J. Spiller: On Artificial Magnetite.—W. Symons: Remarks on a paper by the Marquis of Salisbury on Spectral Lines of Cold Temperature.—A. Tribe: Spec. gr. bottle for liquids spontaneously inflammable in contact with air.

SECTION C.—Rev. J. F. Blake: Additional Remains of Pleistocene Mammals in Yorkshire.—W. Blandford: Some Evidences of Glacial Action in Tropical India.—A. Leith Adams: Concluding Report of the Malta Fossil Elephants.—R. Russell: Geological Sketch of Bradford and the neighbourhood.—J. Hopkinson: On Graptolites found (1) in Ramsay Island, St. David's; (2) in the Ludlow Rocks of Shropshire.—H. Hicks: On the Arenig and Llandeilo Rocks of St. David's.—J. L. Lobley: On the British Palæozoic Arcade.

SECTION D.—Hyde Clarke: Comparative Chronology of Man in America in relation to Comparative Philology.—Prehistoric Names of Weapons.—W. T. Blandford: The Fauna of Persia.—J. Willis: The Flora of the Environs of Bradford.—J. Milnes Fothergill: Heart and Brain.—K. Kaines: A true Cerebral Theory necessary to Anthropology.

SECTION E.—C. F. Beke: On the True Position of Mount Sinai.—W. Blandford: Physical Geography of the Deserts of Persia and Central Asia.—G. Darwin: On Some Maps of the World and on a Portable Globe.—Rev. W. B. Kerr: Overland Route from India.—E. L. Oxenham: A Journey from Pekin to Hankow.—Capt. Davis: The Voyage of the *Challenger*.—Sir F. Goldsmid: On Persia.

SECTION F.—Hyde Clarke: The Influence of Large Centres of Population on Intellectual Manifestation.—Dr. Appleton: On some of the Economical Aspects of Endowments of Education and Original research.—T. G. P. Hallett: The Income Tax Question.—W. P. Henderson: Commercial Panics.—W. Hastings: Postal Reform.—R. H. Palgrave: The Relation of the Banking Reserve of the Bank of England to the Current Rate of Interest.—G. C. T. Barnsley: The Poor-Law Board and its Effect on Thrift.

Among British men of science expected to be present at this year's meeting are the following:—Prof. W. G. Adams, F.R.S.; Major-General Sir J. Alexander, Sir Rutherford Alcock, K.C.B.; Prof. Attfield, Prof. R. S. Ball, Admiral Sir E. Belcher, K.C.B.; W. H. Barlow, F.R.S.; Prof. Balfour, W. Boyd Dawkins, F.R.S.; Sir P. G. Egerton, F.R.S.; Sir W. Fairbairn, F.R.S.; Dr. W. Farr, Prof. Michael Foster, M.D.: Mr. J. G. Fitch, Mr. P. Le Neve Foster, Mr. C. L. N. Foster, Col. Lane Fox, Sir G. D. Gibb, Bart.; Rev. Prof. Griffiths, Capt. D. Galton, C.B.; G. Griffiths, F.C.S.; Prof. Greenwood, Mr. J. W. L. Glaisher, Sir F. Goldsmid, J. P. Gassiot, F.R.S.; Dr. J. H. Gladstone, F.R.S.; Dr. P. H. Holland, W. Huggins, D.C.L., F.R.S.; Prof. Hughes, Lord Houghton, F.R.S.; Prof. G. Harley, F.R.S.; Prof. Herschel, Rev. R. Harley, F.R.S.; Mr. A. V. Harcourt, F.R.S.; Mr. G. J. Holyoake, Mr. A. K. Johnston, Prof. Leone Levi, Prof. J. Clerk Maxwell, F.R.S.; Prof. A. Newton, F.R.S.; Vice-Admiral Ommaney, C.B., F.R.S.: Prof. Phillips, W. Pengelly, F.R.S.; the Earl of Rosse, Prof. G. Rolleston, M.D., F.R.S.; Prof. Roscoe, F.R.S.; Dr. W. Rutherford, Dr. W. J. Russell, F.R.S.; Prof. Savage, Prof. Balfour Stewart, F.R.S.; Major-General Scott, Prof. Smith, Prof. Tyndall, F.R.S.; Prof. W. C. Williamson, F.R.S.; T. Wright, F.S.A.; Prof. Williamson, F.R.S.; the Archbishop of York, &c. The following foreigners are also expected to be present:—M. Guido Cora, Dr. Janssen, Prof. Klein, Baron von Richthofen, Arminius Vambery, &c.

INAUGURAL ADDRESS OF PROF. ALEXANDER W. WILLIAMSON, F.R.S., PRESIDENT.

INSTEAD of rising to address you on this occasion I had hoped to sit quietly amongst you, and to enjoy the intellectual treat of listening to the words of a man of whom England may well be proud—a man whose life has been spent in reading the great book of nature, for the purpose of enriching his fellow men with a knowledge of its truths—a man whose name is known and honoured in every corner of this planet to which a knowledge of science has penetrated—and, let me add, a man whose name will live in the grateful memory of mankind as long as the records of such noble work are preserved.

At the last meeting of the Association I had the pleasure of proposing that Dr. Joule be elected President for the Bradford Meeting, and our Council succeeded in overcoming his reluctance and in persuading him to accept that office.

Nobly would Joule have discharged the duties of President had his bodily health been equal to the task; but it became apparent after a while that he could not rely upon sufficient strength to justify him in performing the duties of the Chair, and, in obedience to the orders of his physician, he placed his resignation in the hands of the Council about two months ago. When, under these circumstances, the Council did me the great honour of asking me to accept their nomination to the Presidentship, I felt that their request ought to have with me the weight of a command.

For a good many years past Chemistry has been growing at a more and more rapid rate, growing in the number and variety of facts which are added to its domain, and not less remarkably in the clearness and consistency of the ideas by which these facts are explained and systematised. The current literature of chemical research extends each year to the dimensions of a small library; and mere brief extracts of the original papers published annually by the Chemical Society, partly aided by a grant from this Association, take up the chief part of a very stout volume. I could not, if I would, give you to-night even an outline of the chief newly discovered compounds and of the various changes which they undergo, describing each of them by its own name (often a very long one) and recording the specific properties which give to each substance its highest scientific interest. But I am sure that you would not wish me to do so if I could; for we do not meet here to study chemistry; I conceive that we meet here for the purpose of considering what this wondrous activity in our science means, what is the use of it, and, true to our object as embodied in the name of this Association, to consider what we can do to promote the Advancement of Science. I propose to lay before you some facts bearing on each of these questions, and to submit to you some considerations respecting them.

In order to ascertain the meaning of the work which has been going on in chemistry, it will, I think, be desirable for us to consider the leading ideas which have been in the minds of chemists, and which guide their operations.

Now, since the father of modern chemistry, the great Dalton, gave to chemists a firm hold of the idea of Atoms, their labours have been continually guided by that fundamental idea, and have confirmed it by a knowledge of more and more facts, while at the same time steadily adding to our knowledge of the properties of atoms. Every chemist who is investigating a new compound takes for granted that it must consist of a great number of atom-clusters (called by him molecules), all of them alike, and each molecule consisting of a certain number of atoms of at least two kinds. One of his first endeavours is to ascertain how many atoms of each kind there are in each molecule of the compound. I must not attempt to describe to you the various kinds of experiment which he performs for the purpose of getting this information, how each experiment is carried out with the aid of delicate instruments and ingenious contrivances found by long experience to enable him to obtain the most trustworthy and accurate results; but I want to draw your attention to the reasoning by which he judges of the value of such experiments when they agree among themselves, and to the meaning which he attaches to their result.

If the result of his experiments does not nearly agree with any atomic formula (that is, if no conceivable cluster of atoms of the kinds known to be in the compound would on analysis give such results as those obtained), the chemist feels sure that his experiments must have been faulty: either the sample of substance which he worked upon contained foreign matter, or his analyses were not made with due care. He sets to work again, and goes on till he arrives at a result which is consistent with his knowledge of the combining-properties of atoms. It is hardly necessary to say that even the best experiment is liable to error, and that even a result obtained with the utmost care cannot be expected to afford more than an approximation to the truth. Every good analysis of a pure compound leads to results which approximate to those required by the Atomic Theory; and chemists trust so thoroughly to the truth of that guide, that they correct the results of such analysis by the aid of it.

The chemical idea of atoms serves for two purposes:—

1. It gives a clear and consistent explanation of an immense number of facts discovered by experiment, and enables us to compare them with one another and to classify them.

2. It leads to the anticipation of new facts, by suggesting new compounds which may be made; at the same time it teaches us that no compounds can exist with their constituents in any other than atomic proportions, and that experiments which may imply the existence of any such compound are faulty.

We have the testimony of the great Berzelius to the flood of light which the idea of atoms at once threw on the facts respecting combining proportions which had been accumulated before it was made known; and from that time forward its value has rapidly increased as each succeeding year augmented the number of facts which it explained.

Allow me at this point of my narrative to pause for a moment in order to pay a tribute of respect and gratitude to the memory of one who has recently passed from among us, and who in the time of his full activity was a leader of the discoveries of new facts in the most difficult part of our science. Liebig has been generally known in this country through his writings on agricultural chemistry, through his justly popular letters on chemistry, and other writings, by means of which his brilliant intellect and ardent imagination stimulated men to think and to work. Among chemists he was famed for his numerous discoveries of new organic compounds, and their investigation by the aid of improved methods; but I believe that the greatest service which his genius rendered to science was the establishment of the chemical school of Giessen, the prototype of the numerous chemical schools for which Germany is now so justly celebrated. I think it is not too much to say that the Giessen laboratory, as it existed some thirty years ago, was the most efficient organisation for the promotion of chemistry which had ever existed.

Picture to yourselves a little community of which each member was fired with enthusiasm for learning by the genius of the great master, and of which the best energies were concentrated on the one object of experimental investigation.

The students were for the most part men who had gone through a full curriculum of ordinary studies at some other University, and who were attracted from various parts of the world by the fame of this school of research.

Most of the leading workers of the next generation were pupils of Liebig; and many of them have established similar schools of research.

We must not, however, overlook the fact that Liebig's genius and enthusiasm would have been powerless in doing this admirable work, had not the rulers of his Grand-Duchy been enlightened enough to know that it was their duty to supply him with the material aids requisite for its successful accomplishment.

Numberless new compounds have been discovered under the guidance of the idea of atoms; and in proportion as our knowledge of substances and of their properties became more extensive, and our view of their characteristics more accurate and general, were we able to perceive the outlines of their natural arrangement, and to recognise the distinctive characteristics of various classes of substances. I wish I could have the pleasure of describing to you the origin and nature of some of these admirable discoveries, such as homologous series, types, radicals, &c.; but it is more to our purpose to consider the effect which they had upon the idea of atoms, an idea which, still in its infancy, was plunged into the intellectual turmoil arising from a variety of novel and original theories suggested respectively by independent workers as best suited for the explanation of the particular phenomena to which their attention was mainly directed.

Each of these workers was inclined to attach quite sufficient importance to his own new idea, and to sacrifice for its sake any other one capable of interfering with its due development.

The father of the atomic theory was no more; and the little infant had no chance of life, unless from its own sterling merits it were found useful in the work still going on.

What then was the result? Did it perish like an ephemeral creation of human fancy? or did it survive and gain strength by the inquiries of those who questioned Nature and knew how to read her answers?

Although anticipating my answer to these questions, you will probably be surprised to hear the actual result which I have to record, a result so wonderful that the more I think of it the more I marvel at it. Not only did these various theories contain nothing at variance with the atomic theory; they were found to be natural and necessary developments of it, and to serve for its application to a variety of phenomena which were unknown to its founder.

Among the improvements of our knowledge of atoms which have taken place, I ought to mention the better evaluations of the relative weight of atoms of different kinds, which have been made since Dalton's time. More accurate experiments than

those which were then on record have shown us that certain atoms are a little heavier or lighter than was then believed, and the work of perfecting our observations is constantly going on with the aid of better instruments and methods of operation. But, apart from these special corrections, a more sweeping change has taken place, not in consequence of more accurate experiments interpreted in the usual way, but in consequence of a more comprehensive view of the best experimental results which had been obtained, and a more consistent interpretation of them. Thus the atomic weight of carbon had been fixed at 6 by Dumas's admirable experiments; and it was quite conceivable that a still more perfect determination might slightly increase or diminish this number. But those who introduced the more sweeping change asserted in substance that two of these supposed atoms, whatever may be the precise weight of each, always are together and never separate from one another; and they accordingly applied the term atom to that indivisible mass of carbon weighing twice as much as a carbon atom had been supposed to weigh. So also with regard to other elements, it has been shown that many atoms are really twice as heavy as had been supposed, according to the original interpretation of the best experiments. This change was brought about by what I may be permitted to call the operation of stock-taking. Dalton first took stock of our quantitative facts in a business-like manner; but the amount and variety of our chemical stock increased so enormously after his time, that the second stock-taking absorbed the labours of several men for a good many years. They were men of different countries and very various turns of mind; but, as I mentioned just now, they found no other fundamental idea to work with than Dalton's; and the result of their labours has been to confirm the truth of that idea and to extend greatly its application.

One of the results of our endeavours to classify substances according to their natural resemblances has been the discovery of distinct family relationships among atoms, each family being distinguished by definite characteristics. Now among the properties which thus characterise particular families of atoms, there is one of which the knowledge gradually worked out by the labours of an immense number of investigators must be admitted to constitute one of the most important additions ever made to our knowledge of these little masses.

I will endeavour to explain it to you by a simple example. An atom of chlorine is able to combine with one atom of hydrogen or one atom of potassium; but it cannot combine with two atoms. An atom of oxygen, on the other hand, can combine with two atoms of hydrogen or with two atoms of potassium, or with one atom of hydrogen and one of potassium; but we cannot get it in combination with one atom of hydrogen or of potassium solely.

Again, an atom of nitrogen is known in combination with three atoms of hydrogen; while an atom of carbon combines with four of hydrogen. Other atoms are classified, from their resemblance to these respectively, as Monads, Dyads, Triads, Tetrads, &c.

The combining value which we thus recognise in the atoms of these several classes has led us naturally to a consideration of the order in which atoms are arranged in a molecule. Thus, in the compound of oxygen with hydrogen and potassium, each of these latter atoms is directly combined with the oxygen, and the atom of oxygen serves as a connecting link between them. Hydrogen and potassium have never been found capable of uniting directly with one another; but when both combined with one atom of oxygen they are in what may be called indirect combination with one another through the medium of that oxygen.

One of the great difficulties of chemistry some few years ago was to explain the constitution of isomeric compounds, those compounds whose molecules contain atoms of like kinds and in equal numbers, but which differ from one another in their properties. Thus a molecule of common ether contains four atoms of carbon, ten atoms of hydrogen, and one of oxygen. Butylic alcohol, a very different substance, has precisely the same composition. We now know that in the former the atom of oxygen is in the middle of a chain of carbon atoms, whereas in the latter it is at one end of that chain. You might fancy it impossible to decide upon anything like consistent evidence such questions as this; but I can assure you that the atomic theory, as now used by chemists, leads frequently to conclusions of this kind, which are confirmed by independent observers, and command general assent. That these conclusions are, as far as they go, true descriptions of natural phenomena is shown by the fact that each of them serves in its turn as a stepping-stone to further discoveries.

One other extension of our knowledge of atoms I must briefly mention, one which has as yet received but little attention, yet which will, I venture to think, be found serviceable in the study of the forces which bring about chemical change.

The original view of the constitution of molecules was statical; and chemists only took cognizance of those changes of place among their atoms which result in the disappearance of the molecules employed, and the appearance of new moleclules formed by their reaction on one another. Thus, when a solution of common salt (sodic chloride) is mixed with a solution of silver nitrate, it is well-known that the metallic atoms in these respective compounds change places with one another, forming silver chloride and sodic nitrate; for the silver chloride soon settles to the bottom of the solution in the form of an insoluble powder, while the other product remains dissolved in the liquid. But as long as the solution of salt remained undecomposed, each little molecule in it was supposed to be chemically at rest. A particular atom of sodium which was combined with an atom of chlorine was supposed to remain steadily fixed to it. When this inactive solution was mixed with the similarly inactive solution of silver nitrate, the interchange of atoms known to take place between their respective molecules was nominally explained by the force of predisposing affinity. It was, in fact, supposed that the properties of the new compounds existed and produced effects before the compounds themselves had been formed.

I had occasion to point out a good many years ago that molecules which appear to be chemically at rest are acting on one another, when in suitable conditions, in the same kind of way as those which are manifestly in a state of chemical change—that for instance, the molecules of liquid sodic chloride exchange sodium atoms with one another, forming new molecules of the same compound undistinguishable from the first, so that, in an aggregate of like molecules, the apparent atomic rest is the result of the interchange of like atoms between contiguous molecules. Such exchanges of atoms take place not only between molecules of identical composition, but also between contiguous molecules containing different elements. For instance, in a mixture of sodic chloride and potassic iodide an interchange of metallic atoms takes place, forming potassic chloride and sodic iodide. The result of the exchange in such a case is to form a couple of new molecules different from the original couple. But these products are subject to the same general law of atomic changes, and their action on one another reproduces a couple of molecules of the materials.

Thus a liquid mixture formed from two compounds contains molecules of four kinds, which we may describe as the two materials and the two products. The materials are reacting on one another, forming the products; and these products are, in their turn, reacting on one another, reproducing the materials.

If one of the products of atomic exchange between two molecules is a solid while the other remains liquid (as when sodic chloride is mixed with silver nitrate), or if one is gaseous while the other remains liquid, so that the molecules of the one kind cannot react on those of the other kind and reproduce the materials, then the continued reaction of the materials on one another leads to their complete mutual decomposition. Such complete mutual decomposition of two salts takes place whenever they react on one another under such conditions that the products cannot react on one another and reproduce the materials; whereas partial decomposition takes place whenever the materials form a homogeneous mixture with the products.

Now, if in any such homogeneous mixture more exchanges of atoms take place between the materials than between the products, the number of molecules of the products is increased, because more of them are being made than unmade; and reciprocally, if more exchanges of atoms take place between the products than between the materials, the number of molecules of the materials is increased. The mixture remains of constant composition when there are in the unit of time as many decomposing changes as reproducing changes.

Suppose that we were to determine by experiment the proportion between the number of molecules of the materials, and the number of molecules of the products, in a mixture the composition of which remains constant, and that we found, for instance, twice as many of materials as of products; what would this mean? Why, if every two couples of materials only effect in the unit of time as many exchanges as every one couple of pro-

ducts, every couple of materials is only exchanging half as fast as every couple of products.

In fact you perceive that a determination of the proportion in which the substances are present in such a mixture will give us a measure of the relative velocities of those particular atomic motions; and we may thus express our result:—The force of chemical combination is inversely proportional to the number of atomic interchanges.

I cannot quit this part of our subject without alluding to the fact that some few chemists, of such eminence as to be entitled to the most respectful attention, have of late years expressed an opinion that the idea of atoms is not necessary for the explanation of the changes in the chemical constitution of matter, and have sought as far as possible to exclude from their language any allusion to atoms.

It would be out of place on this occasion to enter into any discussion of the questions thus raised; but I think it right to point out:—

I. That these objectors have not shown us any inconsistency in the atomic theory, nor in the conclusions to which it leads.

II. That neither these nor any other philosophers have been able to explain the facts of chemistry on the assumption that there are no atoms, but that matter is infinitely divisible.

III. That when they interpret their analyses, these chemists allow themselves neither more nor less latitude than the atomic theory allows; in fact, they are unconsciously guided by it.

These facts need no comment from me.

Our science grows by the acquisition of new facts which have an intelligible place among our ideas of the order of nature; but in proportion as more and more facts are arranged before us in their natural order, in proportion as our view of the order of nature becomes clearer and broader, we are able to observe and describe that order more fully and more accurately—in fact, to improve our ideas of the order of nature. These more extensive and more accurate ideas suggest new observations, and lead to the discovery of truths which would have found no place in the narrower and less accurate system. Take away from Chemistry the ideas which connect and explain the multifarious facts observed, and it is no longer a science; it is nothing more than a confused and useless heap of materials.

The answer to our question respecting the meaning of the earnest work which is going on in our science must, I think, now be plain to you. Chemists are examining the combining properties of atoms, and getting clear ideas of the constitution of matter.

Admitting, then, for the present, that such is the meaning of chemical work, we have to consider the more important question of its use; and I think you will agree with me that, in order to judge soundly whether and in what manner such a pursuit is useful, we have to consider its effect upon Man. What habits of mind does it engender? What powers does it develop? Does it develop good and noble qualities and aspirations, and tend to make men more able and more anxious to do good to their fellow-men? Or is it a mere idle amusement, bearing no permanent fruits of improvement?

You will, I think, answer these questions yourselves if I can succeed in describing to you some of the chief qualities which experience has shown to be requisite for the successful pursuit of Chemistry, and which are necessarily cultivated by those who qualify themselves for such a career.

One of the first requirements on the part of an investigator is accuracy in observing the phenomena with which he deals. He must not only see the precise particulars of a process as they present themselves to his observation; he must also observe the order in which these particular appearances present themselves under the conditions of each experiment. No less essential is accuracy of memory. An experimental inquirer must remember accurately a number of facts; and he needs to remember their mutual relations, so that one of them when present to his mind may recall those others which ought to be considered with it. In fact, he cultivates the habit of remembering facts mainly by their place in nature. Accuracy in manual operations is required in all experimental inquiries; and many of them afford scope for very considerable skill and dexterity.

These elementary qualities are well known to be requisite for success in experimental science, and to be developed by careful practice of its methods; but some higher qualities are quite as necessary as these in all but the most rudimentary manipulations, and are developed in a remarkable degree by the higher work of science.

Thus it is of importance to notice that a singularly good training in the accurate use of words is afforded by experimental chemistry. Everyone who is about to enter on an inquiry, whether he be a first-year's student who wants to find the constituents of a common salt, or whether he be the most skilled and experienced of chemists, seeks beforehand to get such information from the records of previous observations as may be most useful for his purpose. This information he obtains through the medium of words; and any failure on his part to understand the precise meaning of the words conveying the information requisite for his guidance is liable to lead him astray. Those elementary exercises in analytical chemistry, in which brief directions to the students alternate with their experiments and their reports of experiments made and conclusions drawn, afford a singularly effective training in the habit of attending accurately to the meaning of words used by others, and of selecting words capable of conveying without ambiguity the precise meaning intended. Any inaccuracy in the student's apprehension of the directions given, or in the selection of words to describe his observations and conclusions, is at once detected when the result to which he ought to have arrived is known beforehand to the teacher.

Accuracy of reasoning is no less effectively promoted by the work of experimental chemistry. It is no small facility to us that the meaning of the words which we use to denote properties of matter and operations can be learnt by actual observation. Moreover each proposition comprised in chemical reasonings conveys some distinct statement susceptible of verification by similar means; and the validity of each conclusion can be tested, not only by examining whether or not it follows of necessity from true premisses, but also by subjecting it to the independent test of special experiment.

Chemists have frequent occasion to employ arguments which indicate a probability of some truth; and the anticipations based upon them serve as guides to experimental inquiry by selecting crucial tests. But they distinguish most carefully such hypotheses from demonstrated facts.

Thus a pale green solution, stated to contain a pure metallic salt, is found to possess some properties which belong to salts of Iron. Nothing else possesses these properties except salts of Nickel; and they manifest a slight difference from Iron salts in one of the properties observed.

The analyst could not see any appearance of that peculiarity which distinguishes Nickel salts; so he concludes that he has probably got iron in his solution, but almost certainly either Iron or Nickel. He then makes an experiment which will, he knows, give an entirely different result with Iron salts and Nickel salts; and he gets very distinctly the result which indicates Iron.

Having found in the green liquid properties which the presence of Iron could alone impart, he considers it highly probable that Iron is present. But he does not stop there; for, although the facts before him seem to admit of no other interpretation, he knows that, from insufficient knowledge or attention, mistakes are sometimes made in very simple matters. The analyst therefore tries as many other experiments as are known to distinguish Iron salts from all others; and if any one of these leads distinctly to a result at variance with his provisional conclusion, he goes over the whole inquiry again, in order to find where his mistake was. Such inquiries are practised largely by students of chemistry, in order to fix in their minds, by frequent use, a knowledge of the fundamental properties of the common elements, in order to learn by practice the art of making experiments, and, above all, in order to acquire the habit of judging accurately of evidence in natural phenomena. Such a student is often surprised at being told that it is not enough for him to conduct his experiments to such a point that every conclusion except one is contrary to the evidence before him—that he must then try every confirmatory test which he can of the substance believed to be present, and ascertain that the sample in his hands agrees, as far as he can see, in all properties of the known substance of which he believes it to be a specimen.

Those who tread the path of original inquiry, and add to human knowledge by their experiments, are bound to practise this habit with the most scrupulous fidelity and care, or many and grave will be the mistakes they will make.

Thus a chemist thinks it probable that he might prepare some well-known organic body of the aromatic family by a new process. He sets to work and obtains a substance agreeing in appearance, in empirical composition, in molecular weight, and in many other properties with the compound which he has in view. He is, however, not satisfied that his product is a sample of that

compound until he has examined carefully whether it possesses all the properties which are known to belong to the substance in question. And many a time is his caution rewarded by the discovery of some distinct difference of melting-point, or of crystalline form, &c., which proves that he has made a new compound isomeric with the one which he expected to make. It seemed probable, from the agreement of the two substances in many particulars, that they might be found to agree in all, and might be considered to be the same compound; but complete proof of that conclusion consists in showing that the new substance agrees with all that we know of the old one.

In the most various ways chemists seek to extend their knowledge of the uniformity of nature; and their reasonings by analogy from particulars to particulars suggest the working hypotheses which lead to new observations. Before, however, proceeding to test the truth of his hypothesis by experiment, the chemist passes in review, as well as he can, all the general knowledge which has any bearing on it, in order to find agreement or disagreement between his hypothesis and the ideas established by past experience. Sometimes he sees that his hypothesis is at variance with some general law in which he has full confidence, and he throws it aside as disproved by that law. On other occasions he finds that it follows of necessity from some known law, and he then proceeds to verify it by experiment, with a confident anticipation of the result. In many cases the hypothesis does not present sufficiently distinct agreement or disagreement with the ideas established by previous investigations to justify either the rejection of it or a confident belief in its truth; for it often happens that the results of experience of similar phenomena are not embodied in a sufficiently definite or trustworthy statement to have any other effect than that of giving probability or the contrary to the hypothesis.

Another habit of mind which is indispensable for success in experimental chemistry, and which is taught by the practice of its various operations, is that of truthfulness.

The very object of all our endeavours is to get true ideas of the natural processes of chemical action; for in proportion as our ideas are true do they give us the power of directing these processes. In fact, our ideas are useful only so far as they are true; and he must indeed be blind to interest and to duty who could wish to swerve from the path of truth. But if anyone were weak enough to make the attempt, he would find his way barred by innumerable obstacles.

Every addition to our science is a matter of immediate interest and importance to those who are working in the same direction. They verify in various ways the statements of the first discoverer, and seldom fail to notice further particulars, and to correct any little errors of detail into which he may have fallen. They soon make it a stepping-stone to further discoveries. Anything like wilful misrepresentation is inevitably detected and made known.

It must not, however, be supposed that the investigator drifts unconsciously into the habit of truthfulness for want of temptation to be untruthful, or even that error presents itself to his mind in a grotesque and repulsive garb, so as to enlist from the first his feelings against it; for I can assure you that the precise contrary of these things happens. Error comes before him usually in the very garb of truth, and his utmost skill and attention are needed to decide whether or not it is entitled to retain that garb.

You will easily see how this happens if you reflect that each working hypothesis employed by an investigator is an unproven proposition, which bears such resemblance to truth as to give rise to hopes that it may really be true. The investigator trusts it provisionally to the extent of trying one or more experiments, of which it claims to predict the specific result. Even though it guide him correctly for a while, he considers it still on trial until it has been tested by every process which ingenuity can suggest for the purpose of detecting a fault.

Most errors which an experimentalist has to do with are really imperfect truths, which have done good service in their time by guiding the course of discovery. The great object of scientific work is to replace these imperfect truths by more exact and comprehensive statements of the order of nature.

Whoever has once got knowledge from Nature herself by truthful reasoning and experiment, must be dull indeed if he does not feel that he has acquired a new and noble power, and if he does not long to exercise it further, and make new conquests from the realm of darkness by the aid of known truths.

The habit of systematically searching for truth by the aid of known truths, and of testing the validity of each step by constant reference to Nature, has now been practised for a sufficiently long time to enable us to judge of some of its results.

Every true idea of the order of Nature is an instrument of thought. It can only be obtained by truthful investigation; and it can only be used effectively in obedience to the same laws. But the first idea which is formed of anything occurring in nature affords only a partial representation of the actual reality, by recording what is seen of it from a particular point of view. By examining a thing from different points of view we get different ideas of it; and when we compare these ideas accurately with one another, recollecting how each one was obtained, we find that they really supplement each other.

We try to form in our minds a distinct image of a thing capable of producing these various appearances; and when we have succeeded in doing so, we look at it from the different points of view from which the natural object has been examined, and find that the ideas so obtained meet at the central image. It usually happens that an accurate examination of the mutual bearings of these ideas on a central image suggests additions to them and correction of some particulars in them.

Thus it is that true ideas of a natural phenomenon confirm and strengthen one another; and he who aids directly the development of one of them is sure to promote indirectly the consolidation of others.

Each onward step in the search for truth has made us stronger for the work; and when we look back upon what has been done by the efforts of so many workers simply but steadily directed by truth towards further truth, we see that they have achieved, for the benefit of the human race, the conquest of a systematic body of truths which encourages men to similar efforts while affording them the most effectual aid and guidance.

This lesson of the inherent vitality of truth, which is taught us so clearly by the history of our science, is well worthy of the consideration of those who, seeing that iniquity and falsehood so frequently triumph for a while in the struggle for existence, are inclined to take a desponding view of human affairs, and almost to despair of the ultimate predominance of truth and goodness. I believe it would be impossible at the present time to form an adequate idea of the vast consequences which will follow from the national adoption of systematic measures for allowing our knowledge of truth to develop itself freely, through the labours of those who are willing and able to devote themselves to its service, so as to strengthen more and more the belief and trust of mankind in its guidance, in small matters as well as in the highest and most important considerations.

I am desirous of describing briefly the more important of those measures; but first let me mention another habit of mind which naturally follows from the effective pursuit of truth—a habit which might be described in general terms as the application to other matters of the truthfulness imparted by science.

The words which the great German poet put into the mouth of Mephistopheles when describing himself to Faust afford perhaps the most concise and forcible statement of what we may call the anti-scientific spirit:—

> Ich bin der Geist der stets verneint,
> Dem alles, was entsteht, zuwider ist.

The true spirit of science is certainly affirmative, not negative; for, as I mentioned just now, its history teaches us that the development of our knowledge usually takes place through two or more simultaneous ideas of the same phenomenon, quite different from one another, both of which ultimately prove to be parts of some more general truth; so that a confident belief in one of those ideas does not involve or justify a denial of the others.

I could give you many remarkable illustrations of this law from among ideas familiar to chemists. But I want you to consider with me its bearing on the habit of mind called toleration, of which the development in modern times is perhaps one of the most hopeful indications of moral improvement in man.

In working at our science we simply try to find out what is true; for although no usefulness is to be found at first in most of our results, we know well that every extension of our knowledge of truth is sure to prove useful in manifold ways. So regular an attendant is usefulness upon truth in our work, that we get accustomed to expect them always to go together, and to believe that there must be some amount of truth wherever there is manifest usefulness.

The history of human ideas, so far as it is written in the records of the progress of science, abounds with instances of men contributing powerfully to the development of important general

ideas, by their accurate and conscientious experiments, while at the same time professing an actual disbelief in those ideas. Those records must indeed have been a dead letter to any who could stand carping at the intellectual crotchets of a good and honest worker, instead of giving him all brotherly help in the furtherance of his work.

To one who knows the particulars of our science thoroughly, and who knows also what a variety of ideas have been resorted to in working out the whole body of truths of which the science is composed, there are few more impressive and elevating subjects of contemplation than the unity in the clear and bold outline of that noble structure.

I hope that you will not suppose, from my references to chemistry as promoting the development of these habits and powers of mind, that I wish to claim for that particular branch of science any exclusive merit of the kind; for I can assure you that nothing can be further from my intention.

I conceived that you would wish me to speak of that department of science which I have had occasion to study more particularly; but much that I have said of it might be said with equal truth of other studies, while some of its merits may be claimed in a higher degree by other branches of science. On the other hand, those highest lessons which I have illustrated by chemistry are best learnt by those whose intellectual horizon includes other provinces of knowledge.

Chemistry presents peculiar advantages for educational purposes in the combination of breadth and accuracy in the training which it affords; and I am inclined to think that in this respect it is at present unequalled. There is reason to believe that it will play an important part in general education, and render valuable services to it in conjunction with other scientific and with literary studies.

I trust that the facts which I have submitted to your consideration may suffice to show you how fallacious is that materialistic idea of physical science which represents it as leading away from the study of man's noblest faculties, and from a sympathy with his most elevated aspirations, towards mere inanimate matter. The material work of science is directed by ideas towards the attainment of further ideas. Each step in science is an addition to our ideas, or an improvement of them. A science is but a body of ideas respecting the order of nature.

Each idea which forms part of physical science has been derived from observation of nature, and has been tested again and again in the most various ways by reference to nature; but this very soundness of our materials enables us to raise upon the rock of truth a loftier structure of ideas than could be erected on any other foundation by the aid of uncertain materials.

The study of science is the study of man's most accurate and perfect intellectual labours; and he who would know the powers of the human mind must go to science for his materials.

Like other powers of the mind, the imagination is powerfully exercised, and at the same time disciplined, by scientific work. Every investigator has frequent occasion to call forth in his mind a distinct image of something in nature which could produce the appearances which he witnesses, or to frame a proposition embodying some observed relation; and in each case the image or the proposition is required to be true to the materials from which it is formed. There is perhaps no more perfect elementary illustration of the accurate and useful employment of the imagination than the process of forming in the language of symbols, from concrete data, one of those admirable general propositions called equations; on the other hand, the contemplation of the order and harmony of nature as disclosed to us by science supplies the imagination with materials of surpassing grandeur and brilliancy, while at the same time affording the widest scope for its effects.

The foregoing considerations respecting the meaning and use of scientific work will, I trust, afford us aid in considering what measures ought to be taken in order to promote its advancement, and what we can do to further the adoption of such measures.

Like any other natural phenomenon, the growth of knowledge in the human mind is favoured and promoted by certain circumstances, impeded or arrested by others; and it is for us to ascertain from experience what those circumstances respectively are, and how the favourable ones can be best combined to the exclusion of the others.

The best and noblest things in this world are the result of gradual growth by the free action of natural forces; and the proper function of legislation is to systematise the conditions most favourable to the free action which is desired.

I shall consider the words "Advancement of Science" as referring to the development and extension of our systematic knowledge of natural phenomena by investigation and research.

The first thing wanted for the work of advancing science is a supply of well qualified workers. The second thing is to place and keep them under the conditions most favourable to their efficient activity. The most suitable men must be found while still young, and trained to the work. Now I know only one really effectual way of finding the youths who are best endowed by nature for the purpose; and that is to systematise and develop the natural conditions which accidentally concur in particular cases, and enable youths to rise from the crowd.

The first of these is that a young man gets a desire for knowledge by seeing the value and beauty of some which he has acquired. When he has got this desire, he exerts himself to increase his store; and every difficulty surmounted increases his love of the pursuit, and strengthens his determination to go on. His exertions are seen by some more experienced man, who helps him to place himself under circumstances favourable to further progress. He then has opportunities of seeing original inquiries conducted, perhaps even of aiding in them; and he longs to prove that he also can work out new truths, and make some permanent addition to human knowledge. If his circumstances enable him to prosecute such work, and he succeeds in making some new observations worthy of publication, he is at once known by them to the community of scientific men, and employed among them.

We want, then, a system which shall give to the young favourable opportunities of acquiring a clear and, as far as it goes, a thorough knowledge of some few truths of nature such as they can understand and enjoy—which shall afford opportunity of further and further instruction to those who have best profited by that which has been given to them, and are anxious to obtain more—which shall enable the best students to see what original investigation is, and, if possible, to assist in carrying out some research—and, finally, which shall supply to each student who has the power and the will to conduct researches, all material conditions which are requisite for the purpose.

But investigators, once found, ought to be placed in the circumstances most favourable to their efficient activity.

The first and most fundamental condition for this is, that their desire for the acquisition of knowledge be kept alive and fostered. They must not merely retain the hold which they have acquired on the general body of their science; they ought to strengthen and extend that hold, by acquiring a more complete and accurate knowledge of its doctrines and methods; in a word, they ought to be more thorough students than during their state of preliminary training.

They must be able to live by their work, without diverting any of their energies to other pursuits; and they must feel security against want in the event of illness or old age.

They must be supplied with intelligent and trained assistants to aid in the conduct of their researches, and whatever buildings, apparatus, and materials may be required for conducting those researches effectively.

The desired system must therefore provide arrangements favourable to the maintenance and development of the true student-spirit in investigators while providing them with permanent means of subsistence, sufficient to enable them to feel secure and tranquil in working at science alone, yet not sufficient to neutralise their motives for exertion; and at the same time it must give them all external aids, in proportion to their wants and powers of making good use of them.

Now I propose to describe the outlines of such a system, framed for the sole purpose of promoting research, and then to consider what other results would follow from its working.

If it should appear possible to establish a system for the efficient advancement of science, which would be productive of direct good to the community in other important ways, I think you will agree with me that we ought to do all we can to promote its adoption.

Let the most intelligent and studious children from every primary school be sent, free of expense, to the most accessible secondary school for one year; let the best of these be selected and allowed to continue for a second year, and so on, until the *élite* of them have learnt all that is to be there learnt to advantage. Let the best pupils from the secondary schools be sent to a college of their own selection, and there subjected to a similar process of annual weeding; and, finally, let those who get satisfactorily to the end of a college curriculum be supplied with an allowance sufficient for their maintenance for a year, on condition of their devoting their undivided energies to research, under

the inspection of competent college authorities, while allowed such aids and facilities as the college can supply, with the addition of money-grants for special purposes. Let all who do well during this first year be allowed similar advantages for a second and even a third year.

Each young investigator thus trained must exert himself to obtain some appointment, which may enable him to do the most useful and creditable work of which he is capable, while combining the conditions most favourable to his own improvement.

Let there be in every college as many Professorships and Assistantships in each branch of science as are needed for the efficient conduct of the work there going on, and let every Professor and Assistant have such salary and such funds for apparatus, &c., as may enable him to devote all his powers to the duties of his post, under conditions favourable to the success of those duties; but let each professor receive also a proportion of the fees paid by his pupils, so that it may be his direct interest to do his work with the utmost attainable efficiency, and attract more pupils.

Let every college and school be governed by an independent body of men, striving to increase its usefulness and reputation, by sympathy with the labours of the working staff, by material aid to them when needed, and by getting the very best man they can, from their own or any other college, to supply each vacancy as it arises.

In addition to colleges, which are and always have been the chief institutions for the advancement of learning, establishments for the observation of special phenomena are frequently needed, and will doubtless be found desirable in aid of a general system for the advancement of science.

Now, if a system fulfilling the conditions which I have thus briefly sketched out were once properly established on a sufficient scale, it ought to develop and improve itself by the very process of its working; and it behoves us, in judging of the system, to consider how such development and improvement would come about.

The thing most needed at the present time for the advancement of science is a supply of teachers devoted to that object—men so earnestly striving for more knowledge and better knowledge as to be model students, stimulating and encouraging those around them by their example as much as by their teaching. Young men do not prepare themselves in any numbers for such a career:—

I. Because the chief influences which surround them at school and at college are not calculated to awaken in them a desire to obtain excellence of such kind.

II. Because they could not expect by means of such qualities to reach a position which would afford a competent subsistence.

Let these conditions be reversed, to the extent that existing teachers have powerful inducements to make their students love the study of science for its own sake, with just confidence that they will be able to earn a livelihood if they succeed in qualifying themselves to advance science, and the whole thing is changed. The first batch of young investigators will be dispersed among schools and colleges according to their powers and acquirements, and will improve their influence upon the pupils, and enable them to send up a second batch better trained than the first. This improvement will go on increasing, if the natural forces which promote it are allowed free play: and the youth of each successive generation will have better and more frequent opportunities of awakening to a love of learning, better help and guidance in their efforts to acquire and use the glorious inheritance of knowledge which had been left them, better and more numerous living examples of men devoting their whole lives to the extension of the domain of truth, and seeking their highest reward in the consciousness that their exertions have benefited their fellow-men, and are appreciated by them.

A young man who is duly qualified for the work of teaching the investigation of some particular branch of science, and who wishes to devote himself to it, will become a member of an association of men selected for their known devotion to learning, and for their ability to teach the methods of investigation in their respective subjects. Around this central group is ranged a frequently changing body of youths who trust to them for encouragement and guidance in their respective studies.

Our young investigator finds it necessary to study again more carefully many parts of his subject, and to examine accurately the evidence of various conclusions which he had formerly adopted, in order that he may be able to lead the minds of his pupils by easy and natural yet secure steps to the discovery of the general truths which are within their reach. He goes over his branch of science again and again from the foundation upwards striving each time to present its essential particulars more clearly and more forcibly, arranging them in the order best calculated to stimulate an inquiring mind to reflect upon their meaning, and to direct its efforts effectively to the discovery of the general ideas which are to be derived from them. He is encouraged in these efforts by the sympathy of his colleagues, and often aided by suggestions derived from their experience in teaching other branches of science, or by information respecting doctrines or methods which throw a light upon those of his own subject.

No known conditions are so well calculated to give a young investigator the closest and strongest grasp of his object of which he is capable as those in which he is placed while thus earnestly teaching it in a college; and inasmuch as a thorough mastery of known truths is needed by everyone who would work to advantage at the discovery of new truths of that kind, it will, in most cases, be an object of ambition to the ablest young investigators to get an opportunity of going through the work of teaching in a college, in order to improve themselves to the utmost for the work of original research. There is, however, another advantage to them in having such work to do; for the best way to ascertain at any one time what additions may be made to a science, is to examine the facts which have been discovered last, and to consider how far they confirm and extend the established ideas of the science, how far they militate against those ideas. An investigating teacher is constantly weaving new facts into the body of his science, and forming anticipations of new truths by considering the relation of these new facts to the old ones.

When our investigator has thus got a thorough mastery of his science and new ideas for its extension, he ought to have the opportunity of turning his improved powers to account by devoting more of his time to original research; in fact he ought to teach research by example more than hitherto, and less by elementary exercises upon known facts. If he has discharged the duties of his first post with manifest efficiency, he will be promoted, either in his own or some other college, to a chair affording more leisure and facility for original research by his own hands and by those of his assistants and pupils. Some investigators may find it desirable to give up after a while all teaching of previously published truths, and confine themselves to guiding the original researches of advanced pupils, while stimulating them by the example of their own discoveries. But most of them will probably prefer to do elementary teaching work from time to time, for the sake of the opportunity of going over the groundwork of their science, with a knowledge of the new facts and enlarged ideas recently established.

Now it must be observed that such a system as the above, once developed to its proper proportions, so as to send annually to secondary schools many thousands of poor children who would otherwise never enjoy such advantages, and so as to train to original investigation a corresponding proportion of them, would not only provide more young investigators than would be needed for systematic teaching functions, but would also give a partial training of the same kind to many whose abilities proved to be insufficient, or whose tastes were not congenial to such pursuit. Some would be tempted by an advantageous opening in an industrial pursuit or in the public service to break off their studies before completion, and others would find, after completing their training, a position of that kind more desirable or more attainable than a purely scientific appointment. Not only would much good of other kinds be accomplished by this circumstance, but we may say with confidence that the system could not work with full advantage for its own special purpose of promoting the advancement of science if it did not diffuse a knowledge of the truths and methods of science beyond the circle of teachers.

There is an urgent need of accurate scientific knowledge for the direction of manufacturing processes, and there could not be a greater mistake than to suppose that such knowledge need not go beyond the elementary truths of science. In every branch of manufacture improvements are made from time to time, by the introduction of new or modified processes which had been discovered by means of investigations as arduous as those conducted for purely scientific purposes, and involving as great powers and accomplishments on the part of those who conducted them.

Any manufacturer of the present day who does not make efficient arrangements for gradually perfecting and improving his

processes ought to make at once enough money to retire; for so many are moving onwards in this and other countries, that he would soon be left behind.

It would be well worth while to establish such a system of scientific education for the sake of training men to the habits of mind which are required for the improvement of the manufacturing arts; and I have no doubt that the expense of working the system would be repaid a hundred times over by the increase of wealth of the community; but I only mention this as a secondary advantage of national education.

A system of the kind could not expand to due dimensions, nor could it, once fully established, maintain itself in full activity, without intelligent sympathy from the community; and accordingly its more active-minded members must be taught some good examples of the processes and results of scientific inquiry, before they can be expected to take much interest in the results achieved by inquirers, and to do their share of the work requisite for the success of the system. I need hardly remind you that there are plenty of other strong reasons why some such knowledge of the truths of nature, and of the means by which they are found out, should be diffused as widely as possible throughout the community.

You perceive that in such educational system each teacher must trust to his own exertions for success and advancement; and he will do so if he is sure that his results will be known and compared impartially with those attained by others. Each governing body must duly maintain the efficiency of their school or college, if its support depend in some degree on the evidences of that efficiency; and they will try to improve their school if they know that every improvement will be seen and duly appreciated.

The keystone of the whole structure is the action of the State in distributing funds carefully among schools and colleges proportionally to the evidence of their doing good work, which could not be continued without such aid.

I am inclined to think that the State ought, as far as possible, to confine its educational grants to the purpose of maintaining and continuing good work which is actually being done, and rarely if ever to initiate educational experiments: first, because it is desirable to encourage private exertions and donations for the establishment of schools and colleges upon new systems, or in new localities, by giving the public full assurance that if any new institution establishes its right to existence, by doing good work for a while, it will not be allowed to die off for want of support; and, secondly, because the judicial impartiality required in the administration of public funds, on the basis of results of work, is hardly compatible with an advocacy of any particular means of attaining such results.

On the other hand, experience has shown that special endowments, which tie up funds in perpetuity for a definite purpose, commonly fail to attain their object under the altered circumstances which spring up in later generations, and not unfrequently detract from the efficiency of the institutions to which they are attached, by being used for objects other than those which it is their proper function to promote.

When there is felt to be a real want of any new institution for the promotion of learning, men are usually willing enough to devote time and money to the purpose of establishing it and giving it a fair trial. It is desirable that they should leave the State to judge of their experiment by its results, and to maintain it or not, according to the evidences of its usefulness. No institution ought, for its own sake, to have such permanent endowments as might deprive its members of motives for exertion.

The State could not, however, discharge these judicial functions without accurate and trustworthy evidence of the educational work done at the various schools and of its success. For this purpose a record must be kept by or under the direction of every teacher of the weekly progress of each pupil, showing what he has done and how he has done it. Official inspectors would have to see to these records being kept upon a uniform scale, so that their results might be comparable. The habit of keeping such records conduces powerfully to the efficiency of teachers; and, for the sake of the due development of the teaching system, it ought to prevail generally. Having such full and accurate means of knowing what opportunities of improvement pupils have enjoyed, and what use they have made of those opportunities, Government ought to stimulate their exertions and test their progress by periodical examinations. It is of the utmost importance to allow any new and improved system of instruction to develop itself freely, by the exertions of those who are willing to undertake the labour and risk of trying it on a practical scale; and the pupils who acquire upon such new system a command of any branch of science, ought to have a fair opportunity of showing what they have achieved and how they have achieved it. An able and impartial examiner, knowing the new systems in use, will encourage each candidate to work out his results in the manner in which he has been taught to work out results of the kind.

Examinations thus impartially conducted with a view of testing the success of teachers in the work which they are endeavouring to do, have a far higher value, and consequent authority, than those which are conducted in ignorance or disregard of the process of training to which the candidates have been subjected; and we may safely say that the examination system will not attain its full usefulness until it is thus worked in intimate connection with a system of teaching.

In order to give every one employed in the educational system the utmost interest in maintaining and increasing his efficiency, it is essential that a due measure of publicity be given to the chief results of their respective labours. Schools and colleges ought, to a considerable extent, to be supported by the fees paid by pupils for the instruction received; and every Professor being in part dependent upon the fees of his pupils will have a direct interest in attracting more pupils to his classes or laboratories. The fame of important original investigations of his own or his pupils, published in the scientific journals, is one of the natural means by which a distinguished Professor attracts disciples, and the success of his pupils in after life is another His prospects of promotion will depend mainly on the opinion formed of his powers from such materials as these by the governing bodies of colleges and by the public; for if each college is dependent for success upon the efficiency of its teaching staff, its governing body must do their best to fill up every vacancy as it arises by the appointment of the ablest and most successful Professor whom they can get; and any college which does not succeed in obtaining the services of able men will soon lose reputation, and fall off in numbers.

There are, however, further advantages to the working of the system to be derived from full publicity of all its more important proceedings. It will supply materials for the formation of a sound public opinion respecting the proceedings of the authorities in their various spheres of action. A claim for money might be made upon Government by the rulers of some college upon inadequate grounds; or a just and proper claim of the kind might be disregarded by Government. Neither of these things will be likely to happen very often if the applications, together with the evidence bearing on them, are open to public scrutiny and criticism; and when they do occasionally happen, there will be a natural remedy for them.

If I have succeeded in making clear to you the leading principles of the plan to be adopted for the advancement of science, including, as it necessarily must do, national education generally, you will, I think, agree with me that, from the very magnitude and variety of the interests involved in its action, such system must of necessity be under the supreme control of Government. Science will never take its proper place among the chief elements of national greatness and advancement until it is acknowledged as such by that embodiment of the national will which we call the Government. Nor can the various institutions for its advancement develop duly their usefulness until the chaos in which they are now plunged gives place to such order as it is the proper function of Government to establish and maintain.

But government has already taken, and is continuing to take action in various matters affecting elementary popular education and higher scientific education, and it would be difficult to arrest such action, even if it were thought desirable to do so. The only practical question to be considered is how the action of Government can be systematised so as to give free play to the natural forces which have to do the work.

By establishing official examinations for appointments and for degrees Government exerts a powerful influence on the teaching in schools and colleges, without taking cognizance, except in some few cases, of the systems of teaching which prevail in them. Again, they give grants of public money from time to time in aid of colleges or universities, or for the establishment of a high school under their own auspices. Sometimes they endow a Professorship. In taking each measure of the kind they are doubtless influenced by evidence that it is in itself a good thing,

calculated to promote the advancement of learning. But a thing which is good in itself may produce evil effects in relation to others, or good effects incommensurate with its cost. Thus examinations afford most valuable aid to educational work when carried on in conjunction with earnest teachers; yet when established in the absence of a good system of education, they are liable to give rise to a one-sided training contrived with a special view of getting young men through the examinations. If no properly educated young men were found for a particular department of the public service, and an examination of all candidates for such appointments were to be established for the purpose of improving the system of training, candidates would consider their power of answering such questions as appeared likely to be set as the condition of their obtaining the appointments, and they would look out for men able and willing to train them to that particular work in as direct and effective a manner as possible. The demand for such instruction would soon be supplied. Some teachers would undertake to give instruction for the mere purpose of enabling candidates to get through the examination; and by the continued habit of such work would gradually come to look upon the examiners as malignant beings who keep youths out of office, and whose vigilance ought to be evaded by such means as experience might show to be most effective for the purpose. Once this kind of direct examination-teaching has taken root, and is known to produce the desired effect of getting young men through the examinations, its existence encourages the tendency on the part of the aandidates to look merely to the examination as the end and aim of their study; and a class of teachers is developed whose exertions are essentially antagonistic to those of the examiners.

There are, no doubt, teachers with a sufficiently clear apprehension of their duty, and sufficient authority, to convince some of the candidates that the proper object of their study should be to increase their power of usefulness in the career for which they are preparing themselves, by thoroughly mastering up to a prescribed point certain branches of knowledge; and that until they had honestly taken the means to do this and believed they had done it effectually, they ought not to go up for examination nor to wish to commence their career.

But it is desirable that all teachers be placed in such circumstances that it may become their interest as well as their duty to co-operate to the utmost of their powers in the object for which the examiners are working. For this purpose their records of the work done under their guidance by each pupil ought to be carefully inspected by the examiners before framing their questians, and ought to be accepted as affording the chief evidence of the respective merits of the pupils.

This is not the place for considering how the general funds for an effective system of national education can best be raised, nor how existing educational endowments can best be used in aid of those funds. It is well known that some colleges of Oxford and Cambridge are possessed of rich endowments, and that many distinguished members of those universities are desirous that the annual proceeds of those endowments should be distributed upon some system better calculated to promote the advancement of learning than that which generally prevails. Indeed we may confidently hope that, true to their glorious traditions, those colleges will be led, by the high-minded and enlightened counsels of their members, to rely upon improving usefulness in the advancement of learhing as the only secure and worthy basis of their action in the use of their funds, so that they may take a leading part in such system of national education as may be moulded out of the present chaos.

But the foundations of a national system of education ought to be laid independently of the present arrangements at Oxford and Cambridge, for we may be sure that the more progress the system makes the more easy will become the necessary reforms in the older universities and colleges.

It is clearly undesirable that Government should longer delay obtaining such full and accurate knowledge of the existing national resources for educational purposes, and of the manner in which they are respectively utilised, as may enable them to judge of the comparative prospects of usefulness presented by the various modes of distributing educational grants. They ought to know what has been done and what is doing in the various public educational establishments before they can judge which of them would be likely to make the best use of a grant of public money.

We have official authority for expecting such impartial administration of educational grants; and it cannot be doubted that, before long, due means will be taken to supply the preliminary conditions.

You are no doubt aware that a Royal Commission was appointed some time ago in consequence of representations made to Government by the British Association on this subject, and it is understood that their instructions are so framed as to direct their particular attention to the manner in which Government may distribute educational grants. The Commission is moreover composed of most distinguished men, and we have every reason to anticipate from their labours a result worthy of the nation and of the momentous occasion.

In speaking of public educational establishments, I refer to those which by their constitution are devoted to the advancement of learning without pecuniary profit to their respective governing bodies. The annual expenditure requisite for keeping up a national system of popular education will necessarily be considerable from the first, and will become greater from year to year; but once Englishmen are fully alive to the parhmount importance of the subject, and see that its attainment is within their reach, we may be sure that its expense will be no impediment. England woul not deserve to reap the glorious fruits of the harvest of knowledge if she grudged the necessary outlay for seed and tillage, were it even ten times greater than it will be. It is no use attempting to establish a national system on any other than a truly national basis. Private and corporate funds inevitably get diverted from popular use, after a few generations, to the use of the influential and rich. A national system must steadily keep in view the improvement of the poor, and distribute public funds each year in the manner best calculated to give to the youths of the poorest classes full opportunities of improvement proportional to their capacities, so that they may qualify themselves for the utmost usefulness to their country of which they are capable. The best possible security for the proper administration of the system will be found in the full and speedy publicity of all the particulars of its working.

It has been frequently remarked that a great proportion of English investigators are men of independent means, who not only seek no advancement as a reward of their labours, but often sacrifice those opportunities of improving their worldly position which their abilities and influence open up to them, for the sake of quietly advancing human knowledge. Rich and powerful men have very great temptations to turn away from science, so that those who devote their time and money to its service prove to us how true and pure a love of science exists in this country, and how Englshmen will cultivate it when it is in their power to do so.

Now and then a youth from the poorer classes is enabled by fortunate accidents and by the aid of a friendly hand to climb to a position of scientific activity, and to give us, as Faraday did, a sample of the intellectual powers which lie fallow in the great mass of the people.

Now, the practical conclusion to which I want to lead you is that it rests with you, who represent the national desire for the advancement of science, to take the onlo measures which can now be taken towards the establishment of a system of education worthy of this country, and adapted to the requirements of science. In the present stage of the business the first thing to be done is to arouse public attention by all practicable means to the importance of the want, and to get people gradually to agree to some definite and practicable plan of action. You will, I think, find that the best way to promote such agreement is to make people consider the natural forces which have to be systematised by legislation, with a view of enabling them to work freely for the desired purpose. When the conditions essential to any national system come to be duly appreciated by those interested in the cause of education, means will soon be found to carry out the necessary legislative enactments.

The highest offices in the State are on our present system filled by men who, whatever their political opinions and party ties, almost infallibly agree in their disinterested desire to signalise their respective terms of office by doing any good in their power. Convince them that a measure desired by the leaders of public opinion is in itself good and useful, and you are sure to carry it.

And, on the other hand, England is not wanting in men both able and willing to come forward as the champions of any great cause, and to devote their best powers to its service.

I may well say this at Bradford after the results achieved by your Member in the Elementary Education Act.

Objections will of course be raised to any system on the score of difficulty and expense, more especially to a complete and

good system. Difficult of realisation it certainly must be, for it will need the devoted and indefatigable exertions of many an able and high-minded man for many a long year. Only show how such exertions can be made to produce great and abiding results, and they will not be wanting. And as for expense, you will surely agree with me that the more money is distributed in such frugal and effective manner, the better for the real greatness of our country.

What nobler privilege is attached to the possession of money than that of doing good to our fellow men? and who would grudge giving freely from his surplus, or even depriving himself of some comforts, for the sake of preparing the rising generation for a life of the utmost usefulness and consequent happiness?

I confidently trust that the time will come when the chief item in the annual budget of the Chancellor of the Exchequer will be the vote for National Education; and when in some later age our nation shall have passed away, when a more true civilisation has grown up and has formed new centres for its throbbing life, when there are but broken arches to tell of our bridges and crumbling ruins to mark the sites of our great cathedrals—then will the greatest and noblest of England's works stand more perfect and more beautiful than ever; then will some man survey the results of Old England's labours in the discovery of imperishable truths and laws of nature, and see that her energy and wealth were accompanied by some nobler attributes—that while Englishmen were strong and ambitious enough to grasp power, they were true enough to use it for its only worthy purpose—that of doing good to others.

I must not, however, trespass longer upon your time and your kind attention. My subject would carry me on, yet I must stop without having done half justice to it.

If I have succeeded in convincing you that a National system of Education is now necessary and possible, and in persuading you to do what you respectively can to prepare the way for it, I shall feel that the first step is made towards that great result.

SECTIONAL PROCEEDINGS

SECTION B.—CHEMICAL SCIENCE.

ADDRESS OF THE PRESIDENT, W. J. RUSSELL, F.R.S.

OF late years it has been the custom of my predecessors in this chair to open the business of the section with an address, and the subject of this address has almost invariably been a review of the progress of chemistry during the past year. I purpose, with your leave, to-day to deviate somewhat from this precedent, and to limit my remarks, as far as the progress of chemistry is concerned, to the history of one chemical substance. The interest and the use of an annual survey, at these meetings, of the progress of chemistry, has to a certain extent passed away, for the admirable extracts of all important chemical papers, now published by the Chemical Society, has in a great measure taken its place, and offers to the chemical student a much more thorough means of learning what progress his science is making than could possibly be done by the study of a presidential address. Doubtless these abstracts of chemical papers are known to others than professional chemists, but I cannot pass them over without recording the great use they have proved to be, how much they have done already in extending in this country an exact knowledge of the progress of science on the continent, and in helping and in stimulating those who are engaged in scientific pursuits in this country. I believe few grants made by this Association have done more real good than those which have enabled the Chemical Society to publish these abstracts.

I dwell for a moment on the doings of the Chemical Society, for I believe in the progress of this Society we have a most important indication of the progress of chemical science in this country. The number of original papers communicated to the Society during the past year has far exceeded that of previous years: during last year fifty-eight papers were read to the Society, whereas the average number for the last three years is only twenty-nine. Further, I may say, there is every appearance of this increased activity not only continuing, but even increasing. Another matter connected with the Society deserves a passing word, I mean its removal from its old rooms at Burlington House, which afforded it very insufficient accommodation, to its new ones in the same building. This transference which is now taking place, will give to the Society a great increase of accommodation, and thus admit of larger audiences attending the lectures, of the proper development of the library, and of the full illustration by experiment of the communications made to it. These improvements must act most beneficially on the Society, and stimulate its future development; even now it numbers some 700 members, and certainly is not one of the least active or least useful of the many scientific societies in London. Since our last meeting, at Brighton, we have lost the most renowned of modern chemists—Liebig. His influence on chemistry through a long and most active life has yet to be written. Publishing his first paper fifty years ago, it is difficult for chemists of the present day to realise the changes in chemical thought, in chemical knowledge, and in chemical experiments which he lived through, and was more than any other chemist active in promoting. His activity was unwearied: he communicated no less than 317 papers to different scientific journals, and almost every branch of chemistry received some impetus from his hand.

Liebig took an active interest in this Association, and I believe the last paper he wrote was one in answer to a communication made at the last meeting of this Association. On two occasions he attended the meetings of the British Association, and has communicated many papers to this section. The meeting at Liverpool in 1837 was the first at which he was present; he there communicated to this section a paper on the products of the decomposition of uric acid, and further gave an account of his most important discovery, made in conjunction with Wöhler, of the artificial formation of urea. At this meeting Liebig was requested to prepare a report on the state of our knowledge of isomeric bodies. This request, although often repeated, was never complied with. He was also requested to report on the state of organic chemistry and organic analysis—thus our section was evidently desirous of giving him full occupation. At the meeting in 1840 at Glasgow, a paper on "Poisons, Contagions, and Miasms," by Liebig was read; it was in fact an abstract of the last chapter in his book on "Chemistry in its applications to Agriculture and Physiology," and the work itself appeared about the same time, dedicated to this Association. Liebig says:—"At one of the meetings of the Chemical Section of the British Association for the advancement of Science, the honourable task of preparing a report upon the state of Organic Chemistry was imposed upon me. In this present work I present the Association with a part of this report." At the next meeting, which was held at Plymouth, in 1841, there was an interesting letter from Liebig to Dr. Playfair read to our section; in it, among other matters, Liebig describes an "excellent method" devised by Drs. Will and Varrentrapp for determining the amount of nitrogen in organic bodies; he also says we have repeated all the expressions of Dr. Brown on the production of silicon from paracyanogen, but we have not been able to confirm one of his results; what our experiences prove is that paracyanogen is decomposed by a strong heat into nitrogen gas, and a residue of carbon which is exceedingly difficult of combustion.

To the next meeting—it was at Manchester, and Dalton was the president of this section—Dr. Playfair communicated an abstract of Professor Liebig's report "On Organic Chemistry applied to Physiology and Pathology." This abstract is printed in our proceedings, and the complete work is looked upon as the second part of the report on Organic Chemistry. This Association may therefore fairly consider that it exercised some influence on Liebig in the production of the most important works that he wrote. Playfair's abstract must have been listened to with the greatest interest, and I doubt not the statements made sharply criticised, specially by the physiologists then at Manchester. Playfair concludes his abstract with these words, thus summing up the special objects of these reports:—"In the opinion of all, Liebig may be considered a benefactor to his species, for the interesting discoveries in agriculture, published by him in the first part of his report. And having in that pointed out means by which the food of the human race may be increased, in the work now before us he follows up the chain in its continuation, and shows how that food may be best adapted to the nutrition of man. Surely there are no two subjects more fitted than these for the contemplation of the philosopher; and by the consummate sagacity with which Liebig has applied to their elucidation the powers of his mind, we are compelled to admit that there is no living philosopher to whom the Chemical Section could have more appropriately entrusted their investigation."

At the meeting at Glasgow, in 1855, Liebig was also present, but then only communicated to this section a short paper on ful-

minuric acid, and some remarks on the use of lime water in the manufacture of bread. Such I believe is the history of the direct relationship which has existed between Liebig and this Association. Indirectly we can hardly recognise how much we owe to him. Interested as he was in the work of this Association I could not but to-day record the instances of direct aid and support which this section has received from him.

I pass on now to the special subject to which I wish to ask your attention.

It is the history of the vegetable colouring matter found in madder. It has been in use from time immemorial, and is still one of the commonest and most important of dyes. It is obtained from a plant largely cultivated in many parts of the world for the sake of the colour it yields, and the special interest which now attaches to it is, that the chemist has lately shown how this natural colouring matter can be made in the laboratory as well as in the fields; how by using a bye-product, which formerly was without value, thousands of acres can be liberated for the cultivation of other crops, and the colouring matter which they formerly produced be cheaper and better prepared in the laboratory or in the manufactory. That a certain colouring matter could be obtained from the roots of the *Rubia tinctorum*, and other species of the same plant, has been so long known, that apparently no record of its discovery remains.

Pliny and Dioscorides evidently allude to it. The former, referring to its value as a dyeing material, says, "It is a plant little known except to the sordid and avaricious, and this because of the large profits obtained from it, owing to its employment in dyeing wool and leather." He further says, "The madder of Italy is the most esteemed, and especially that grown in the neighbourhood of Rome, where and in other places it is produced in great abundance." He further describes it as being grown among the olive-trees, or in fields devoted especially to its growth. The madder of Ravenna, according to Dioscorides, was the most esteemed. Its cultivation in Italy has been continued till the present time, and in 1863 the Neapolitan provinces alone exported it to the value of more than a quarter of a million sterling. At the present day we are all very familiar with this colouring matter as the commonest that is applied to calicoes. It is capable of yielding many colours, such as red, pink, purple, chocolate, and black. The plant in which is the source of this colouring matter is nearly allied botanically and in appearance to the ordinary Galiums, or bed-straws. It is a native probably of Southern Europe as well as Asia. It is a perennial with herbaceous stem, which dies down every year; its square-jointed stalk creeps along the ground to a considerable distance, and the stem and leaves are rough with sharp prickles. The root, which is cylindrical, fleshy, and of a pale yellow colour, extends downwards to a considerable depth. It is from this root, which, when dried, is known as madder, that the colouring matter is obtained. The plant is propagated from suckers or shoots. These require some two or three years to come to full maturity and yield the finest colours, although in France the crop is often gathered after only eighteen months' growth. From its taking so long to develop, it is evidently a crop not adapted to any ordinary series of rotation of crops. The plant thrives best in a warm climate, but has been grown in this country and in the north of Europe.

In India it has been grown from the earliest times, and, as before stated, has been abundantly cultivated in Italy, certainly since the time of Pliny; he also mentions its cultivation in Galilee. In this country its culture has often been attempted, and has been carried on for a short time, but never with permanent success. The madder now used in England is imported from France, Italy, Holland, South Germany, Turkey, and India. In 1857 the total amount imported into this country was 434,056 cwts., having an estimated value of 1,284,989*l*., and the average annual amount imported during the last seventeen years is 310,042 cwts.; while the amount imported last year, 1872, was 283,274 cwts., valued at 922,244*l*. In 1861, it was estimated that in the South Lancashire district alone, 150 tons of madder were used weekly, exclusive of that required for preparing garancine. I quote these figures as showing the magnitude of the industry that we are dealing with. Another point of much interest is the amount of land required for the cultivation of this plant. In England it was found that an acre yielded only from 10 to 20 cwt. of the dried roots, but in South Germany and in France the same amount of land yields about twice that quantity. The madder cultivator digs up the roots in autumn, dries them, in some cases peels them, by beating them with a flail, and exports them in the form of powder, whole root, or, after treatment with sulphuric acid, when it is known as garancine.

The quality of the root varies much, that from the Levant, known as Turkey root, is most valued. According, however, to the colour to be produced, is the madder from one source or another preferred.

To obtain the colouring matter, which is but very slightly soluble in water, from these roots, they are mixed, after being ground, with water in the dye-vessel, and sometimes a little chalk is added. The fabric to be dyed is introduced, and the whole slowly heated; the colouring matter gradually passes from the root to the water, and from the water to the mordanted fabric, giving to it a colour dependent of course on the nature of the mordant.

To trace the chemical history of this colouring matter, we have to go back to the year 1790, when a chemist of the name of Watt precipitated the colouring matter of madder by alum from neutral, alkaline, and acid solutions: he obtained two different colouring matters, but could not isolate them, and many different shades of colours. Charles Batholdi asserted that madder contained much magnesic sulphate, and Hautmann observed the good effect produced on madder by the addition of calcic carbonate. In 1823, F. Kuhlmann made evidently a careful analysis of the madder-root, and describes a red and a fawn colouring matter; but the first really important advance made in our knowledge of the chemical constitution of this colouring matter was by Colin and Robiquet in 1827. They obtained what they believed to be, and what has since really proved to be, the true colouring principle of madder, and obtained it in a state of tolerable purity. Their process for preparing it was very simple. They took Alsace madder in powder, digested it with water, obtaining thus a gelatinous mass, which they treated with boiling alcohol, then evaporated off four-fifths of the alcohol, and treated the residue with a little sulphuric acid, to diminish its solubility. Then, after washing it with several litres of water, they got a yellowish substance remaining. Lastly, they found that on moderately heating this product in a glass tube, they obtained a yellowish vapour formed of brilliant particles, which condensed, giving a distinct zone of brilliant needles, reflecting a colour similar to that from the native lead chromate. They named this substance alizarine, from the Levant name for madder, Alizari, the name by which it is still known there.

A few years later we find other chemists attacking this same subject; in 1831 Gaultier de Claubry and J. Persoz published the account of a long research on the subject; they described two colouring matters, a red and a rose one—the red one was alizarine and the rose one was another body nearly allied to it, and now well known as purpurine. Runge also made an elaborate examination of the madder root; he found no less than five different colouring matters in it—madder-red, madder-purple, madder-orange, madder-yellow, and madder-brown. The first three he considers to be suited for dyeing purposes, but not the last two.

Runge's madder-red is essentially impure alizarin, and his madder-purple impure purpurine. He does not give any analysis of these substances. During the next ten years this subject seems to have attracted but little attention from chemists, but in 1846 Shiel prepared the madder-red and madder-purple of Runge, by processes very similar to those employed by Runge, and analysed these substances. For madder-red he gives the formula $C_{28}H_{18}O_9$, which differs only by H_2O from the formula now adopted. For the madder-purple he gives the formula $C_{28}H_{20}O_{15}$, and for the same substance, after being sublimed, $C_7H_8O_4$. The chemist who has worked most on this subject, and to whom we are principally indebted for what we know with regard to the different constituents contained in the madder root, is Dr. Schunk of Manchester. In Liebig's Annalen for 1848 he gives a long and interesting account of his examination of madder; he isolated and identified several new substances which are most important constituents of the root, and has since this time added much to our knowledge of the chemical constitution of madder. In the paper above alluded to he confirms the presence of the alizarine, and gives to it the formula $C_{14}H_{10}O_4$. The principal properties of this body may best be sketched here. Its volatility and brilliant crystalline appearance have already been mentioned; it is but slightly soluble in cold water, but much more so in alcohol, in ether, and in boiling-water. The colour of its solution is yellow, and when it separates out from a liquid it has a yellow flocculent appearance, differing thus greatly from the red brilliant crystalline substance before described. In order to

obtain this latter body heat had always been used, so until the elaborate experiments of Schunk it was a question whether the heat did not produce a radical change in the substance, whether, in a word, these two bodies were really identical. Schunk's experiments proved that they were, and consequently that this beautiful colouring matter alizarine existed as such in madder. If, however, we go one step further back and examine the fresh root of *Rubia tinctorum*, that is, as soon as it is drawn from the ground, for some time we shall find no trace of alizarine there. On slicing the root it is seen to be of a light carroty colour, and an almost colourless liquid can be squeezed out of it, but this is entirely free from the colouring matters of madder. Let the roots, however, be kept if only for a short time, and then they will give abundant evidence of the presence of alizarine; if simply heated alizarin may be volatilised from them. It appears then that the whole of the tinctorial power of this root is developed after the death of the plant. Schunk explains this curious phenomenon as follows:—That in the cells of the living plant there is a substance which he has isolated, and has named rubian; it is easily soluble in water and in alcohol; the solution is of a yellow colour, and has an intensely bitter taste; when dry it is a hard, brown, gum-like body. It has none of the properties of a dye stuff, but if we take a solution of it, add some sulphuric or hydrochloric acid to it, and boil, a yellow flocculent substance will slowly separate out, and on filtering it off and washing it, it will be found to have the tinctorial properties of madder and to contain alizarine. In the liquid filtered from it there is, with the acid added, an uncrystallisable sugar, so that in this way the original product in the root, the rubian, has apparently been split up into alizarine and into sugar. To apply this reaction to what goes on in the root after its removal from the ground, we have to find if any other substances can take the place of the boiling dilute acid, and Schunk has shown there exists in the root itself a substance which is eminently fitted to produce this splitting up of the rubian. He obtained this decomposing agent from madder simply by digesting it in cold water, and then adding alcohol to the liquid; this threw down a reddish flocculent substance, and if only a small portion of this be added to an aqueous solution of rubian and allowed to stand for a few hours in a warm place, it was found that the rubian was gone, and in place of it there was a thick tenaceous jelly; this, treated with cold water, gave to it no colour, no bitter taste, but much sugar. From the jelly, remaining insoluble, alizarine could be extracted. In fact, of all known substances this very one found in the madder itself is best suited for effecting this decomposition of the rubian.

It appears, then, that these two bodies must exist in the root. The history then seems complete. The two substances are kept apart during the life of the plant in some way of which we know nothing, but as soon as it dies they begin slowly to act on one another, developing thus the colouring matters in madder. It has long been known to dyers that the amount of colouring matter in madder will increase on keeping it; even for years it will go on improving in quality, and an experiment of Schunk's shows that the ordinary madder as used by the dyer has not all the rubian converted into colouring matter, for on taking a sample of it and extracting it with cold water he got an acid solution devoid of dyeing properties, but on allowing this solution to stand some time it gelatinised and then possessed dyeing properties.

Coincident with the appearance of Schunk's first paper was one by Debus on the same subject. He looked upon alizarine as a true acid, and gave it the name of Lizaric acid, but as far as the composition of it was concerned the percentage which he obtained agreed closely with those given by Schunk. One other investigation concludes all that is important in the history of alzarine as obtained from madder. This last research is of great interest; it was by Julius Wolff and Adolph Strecker, and published in 1850. They confirm the results of others so far that there are in the madder root two distinct colouring substances—this important one alizarin, and the other one purpurine. They prepare these colouring matters much in the same way that Schunk did, and very carefully purify and analyse them; the formulæ which they give for them differ, however, from Schunk's: for alizarine they give the formula $C_{20}H_{12}O_6$ and for purpurine $C_{18}H_{12}O_6$. Further, they suggest that by the process of fermentation the former is converted into the latter, and they show that by oxidation they both yielded phthalic acid. Since the publication of this research, until the last year or two, this formula for alizarine has been generally adopted by chemists, and in most modern books we find it given as expressing the true composition of that body. It was not only the careful and elaborate work which they devoted to the subject, but also the ingenious and apparently well-founded theory on the subject which carried conviction with it. Laurent had shown, not many years before, that when naphthalin, that beautiful white crystalline substance obtained from coal tar, was acted on by chlorine, and then treated with nitric acid, a body known as chlornaphthalic acid and having the composition $C_{20}H_{10}Cl_2O_6$ was obtained, and on comparing this formula with the one they had obtained for alizarine, Wolff and Strecker at once concluded that it really was alizarine, only containing two atoms of chlorine in place of two of hydrogen; make this replacement, an operation generally easily performed, and from naphthalin, they had prepared alizarine. Further, this relationship between chlornaphthalic acid, and alizarine is borne out in many ways; it, like alizarine, has the power of combining with different basic substances, has a yellow colour, is insoluble in water, melts at about the same temperature, is volatile, and when acted on by alkalis gives strongly coloured solutions. Taking then all these facts into consideration, can we wonder that these chemists feel convinced that they have established the composition of alizarine, and have shown the source from which it is to be obtained artificially? Apparently but one very simple step remains to crown their work with success, that of replacing the chlorine by hydrogen. Melsens had only shortly before shown how this substitution could easily be made in the case of choracetic acid by acting on it with potassium amalgam, and Kolbe had used the battery for the same purpose. Both these processes, and doubtless all others that the authors can think of, are tried upon the chloronaphthalic acid, but chloronaphthalic acid it remains, and they are obliged to confess they are unable to make this substitution. Still they are strong in the belief that it is to be done and will be done, and conclude the account of their researches by pointing out the great technical advantage it will be to get alizarine from a worthless substance such as naphthalin. One cannot help even now sympathising with these chemists in their not being able to confirm what they had really the strongest evidence for believing must prove to be a great discovery. We now know, however, that had they succeeded in effecting this substitution, or had they in any other way obtained this chloronaphthalic acid without the chlorine, if I may so speak of it, which since their time has been done by Martius and Griess, alizarine would not have been obtained, but a body having a remarkable parallelism in properties to it would have been. This body, like alizarine, is of a yellowish colour, but slightly soluble in water, easily in alcohol and in ether, is volatile, and on oxidation yields the same products, it is, in fact, an analogous body, but belonging to another group. We also know that the formula proposed by Wolff and Strecker, and so long in use, is not the correct one. But little more remains to be added with regard to the history of alizarine as gathered from the study of the natural substance. Schutzenberger and Paraf suggested doubling Wolff and Strecker's formula for alizarine, and Bolley suggested the formula $C_{20}H_{13}O_6$, which owing to the uneven number of hydrogen atoms was soon rejected. If we compare our present knowledge of alizarine with what it was when these researches on the natural product were completed, it is as lightness compared to darkness, and we may well ask whence has come this influx of knowledge? the answer I hope to show you is undoubtedly that it has come from the careful and accurate study of abstract chemistry. I know of no history in the whole of chemistry which more strikingly illustrates how the prosecution of abstract science lays the foundation for great practical improvements. My object now, is then to show you, as shortly as I can, how by indirect means the composition of alizarine was discovered, how it has been built up artificially, and how it is superseding for manufacturing purposes the long-used natural product.

To trace this history from its source we must go back to 1785, when an apothecary of the name of Hofmann obtained the calcium salt of an acid called quinic acid from cinchona bark. This acid is now known to be of common occurrence in plants, it exists in the bilberry and in coffee, in holly, ivy, oak, elm, and ash leaves, and probably many others. Liebig also prepared the calcium salt, and was the first to give a complete analysis of it; the formula he gave for it was $C_{15}H_{24}O_{12}$. Bauss on repeating Liebig's experiments arrived at a somewhat different conclusion, and gave the formula $C_{15}H_{20}O_{10}$. In 1835 at Liebig's suggestion, to determine which formula was correct, Alexander Woskrensky, from St. Petersburg, then a

student at Giessen, undertook the further investigation of this subject, and established the formula $C_{14}H_{24}O_{12}$ the one in fact now in use. In the course of this investigation, which he carried further than merely settling the percentage composition of this acid, he describes what to us now is of most interest, a new substance having peculiar and very marked properties. He says that when a salt of quinic acid is burnt at a gentle heat he gets aqueous vapour, the vapour of formic acid, and a deposit of golden needles which are easily sublimed. Afterwards he describes how this same golden substance may be obtained from any salt of quinic acid by heating it with manganic dioxide and dilute sulphuric acid; it then distils over, condensing in golden yellow needles on the sides of the receiver, and may be rendered pure by resublimation. The composition of this body he finds to be C_3H_2O, and names it quinoyl, a name strongly objected to by Benzelius, as conveying a wrong impression of the nature of the body; he proposes in place of it the name quinone, by which it is still known. Far as this body would seem to be removed from alzarine, yet is the study of its properties which led to the artificial production of alizarine.

Some years afterwards Wöhler also explained them by the decomposition of quinic acid; he prepares again this quinone and follows exactly the process described by Woskrensky. He states that with regard to the properties of this remarkable body he has nothing particular to add. However, he proposes a different formula for it, and discovers and describes other bodies allied to it. Among these is Hydroquinone $C_6H_6O_2$. Laurent afterwards shows that the formula proposed by Wöhler is inconsistent with his and Gerhard's views, and by experiment confirms the former formula for this body. Although many other chemists devoted much attention to this substance, still its real constitution and relation to other compounds remained unknown.

Thus Wöhler, Laurent, Hofmann, Städler, and Hesse, all had worked at it, and much experimental knowledge with regard to it had been acquired. One important point in its history was first the discovery of chloranil by Erdmann in 1841, and then Hofmann, showing that by heating quinone with potassic chlorate and hydrochloric acid chloranil could be obtained from it; that, in fact, chloranil was quinone in which all the hydrogen had been replaced by chlorine. Perhaps the most general impression among chemists was that in constitution it was a kind of aldehyde, certainly its definite place among chemical compounds was unknown.

Kekulé suggests a rational formula for it, but it is to Carl Graebe that we owe our knowledge of its true constitution. In 1868 he published a remarkable and very able paper on the quinone group of compounds, and then first brought forward the view that quinone was a substitution derivative of the hydrocarbon benzol (C_6H_6). On comparing the compounds of these two bodies it is seen that the quinone contains two atoms of oxygen more and two atoms of hydrogen less than benzol, and Graebe, from the study of the decomposition of the quinone, and from the compounds it forms, suggested that the two atoms of oxygen form in themselves a group which is divalent, and thus replace the two atoms of hydrogen. This supposition he very forcibly advocates and shows its simple and satisfactory application to all the then known reactions of this body. This suggestion really proved to be the key, not only to the explanation of the natural constitution of quinone and its derivatives, but to much important discovery besides. At this time quinone seemed to stand alone, no other similarly constituted body was known to exist; but what strikingly confirms the correctness of Graebe's views, and indicates their great value, is that immediately he is able to apply his lately gained knowledge, and to show how other really analogous bodies, other quinones in fact, already exist. He studied with great care this quinone series of compounds and the relation they bore to one another, the relation the hydrocarbon, benzole, bore to its oxidised derivative, quinone, and its relation to the chlorine substitution products derivable from it. At once this seems to have led Graebe to the conclusion, that another such series already existed ready formed, and that its members were well known to chemists, that in fact naphthalin ($C_{10}H_8$) was the parent hydrocarbon and that the chloroxynaphthalin chloride ($C_{10}H_4Cl_2O_2$) and the perchloroxynaphthalin chloride ($C_{10}Cl_6O_2$) were really chlorine substitution compounds of the quinone of this series, corresponding to the bichloroquinone and to chloranil. That the chloroxynaphthalic acid $C_{10}H_4Cl(HO)O_2$ and the perchloroxynaphthalic acid $C_{10}Cl_5(HO)O_2$, all compounds previously discovered by Laurent, were really bodies belonging to that series, and further the supposed isomeric of alizarin discovered by Martius and Griess was really related to this last compound, having the composition $C_{10}H_5(HO)O_2$. Further he was able to confirm this by obtaining the quinone itself of this series, the body having the formula $C_{10}H_6(O_2)$ containing also two atoms less of hydrogen, and two atoms more of oxygen than the hydrocarbon naphthalin, and to the body he gave the characteristic name of naphthoquinone. The chlorine compounds just named are thus chloro-naphthoquinone, or chloroxynaphthoquinone, and correspond to the former chloroquinones, Martius and Griess compound will be an oxynaphthoquinone; many other compositions of this series are also known. Another step confirmatory of this existence of a series of quinones was made by Graebe and Borgmann, as the chloranil could be formed by treating phenol by potassic chlorate and hydrochloric acid and quinone derived from it, they showed that in the next higher series to the phenol series, viz. with cressol, the same reaction held good, and by treating it in the same way they obtain a di- and a tri-chlorotolu-quinone $C_6\begin{cases} CH_3 \\ (O_2)'' \\ Cl_2 \\ H \end{cases}$ $C_6\begin{cases} CH_3 \\ (O_2)'' \\ Cl_3 \end{cases}$ which in physical properties very closely resemble the corresponding compounds in the lower series. Other compounds have also been prepared. In the next step we have the application, which connects these series of discoveries with alizarine. Following the clue of a certain analogy which they believed to exist between the chloranilic acid $C_6Cl_2\genfrac{}{}{0pt}{}{(O_2)''}{(HO)_2}$ and the chloroxynaphthalic acid $C_{10}H_4Cl\genfrac{}{}{0pt}{}{(O)_2}{(HO)_2}$ which they had proved to be quinone compounds and alizarine, believing that a certain similarity of properties indicated a certain similarity of constitution, Graebe and Liebermann were lead to suppose that alizarine must also be a derivative from a quinone, and have the formula $C_{14}H_4\genfrac{}{}{0pt}{}{(O_2)}{(HO_2)}$ This theory they were able afterward to prove; the first thing was to find the hydrocarbon from which the quinone might be derived; this was done by taking alizarine itself, and heating it with a very large excess of zinc powder in a long tube, sealed at one end. A product distilled over, and condensed in the cool part of the tube, and collecting it and purifying it by recrystallisation, they found they had not a new substance, but a hydrocarbon discovered as long ago as 1832 by Dumas and Laurent, and obtained by them from tar. They had given it the formula $C_{15}H_{12}$, and as apparently it thus contained one and a half times as many atoms of carbon and hydrogen as naphthalin did, they named it Paranaphthalin; afterwards Laurent changed its name to Anthracene, by which it is still known. Fritzsche, in 1857, probably obtained the same body, but gave it the formula $C_{14}H_{10}$. Anderson also met with it in his researches, established its composition and found some derivatives from it. Limprich in 1866 showed it could be formed synthetically by heating benzolchloride (C_7H_7Cl) with water and Berthelot has since proved that it is formed by the action of heat on many hydrocarbons. This first step was thus complete and most satisfactory; from alizarin they had obtained its hydrocarbon, and this hydrocarbon was a body already known, and with such marked properties that it was easy to identify it. But would the next requirement be fulfilled, would it like benzol and naphthalin yield a quinone? The experiment had not to be tried, for when they found that anthracene was the hydrocarbon found, they recognised in a body already known to exist, the quinone derivable from it. It had been prepared by Laurent by the action of nitric acid on anthracene, and called by him anthraceneuse, and the same substance was also discovered by Anderson and called by him oxanthracene. The composition of this body was proved by Anderson and Laurent to be $C_{14}H_8O_2$, and it thus bears the same relation to its hydrocarbon anthracene, that quinone and naphthaquinone do to their hydrocarbons. Graebe gave to it the systematic name of anthraquinone. We have then, now, three hydrocarbons C_6H_6, $C_{10}H_8$, and $C_{14}H_{10}$, differing by C_4H_2, and all forming starting points for these different quinone series. Anthroquinone acted upon by chlorine gave substitution products such as might have been foretold. It is an exceedingly stable compound, not attacked even by fusion with potassic hydrate. Bromine does not act upon it in the cold, but at 100° it forms a bibromanthraquinone. Other bromine compounds have also been found. Now, if the analogies which have guided them so far still hold good, they would seem to have the means of forming alizarine artificially. Their theory is

that it is dioxyanthraquinone $C_{14} H_6 \begin{matrix}(O_2)'' \\ (HO)_2\end{matrix}$ and if so, judging from what is known to take place with other quinone derivatives it should be formed from this dibromoanthraquinone on boiling it with potash or soda and then acidulating the solution. They try the experiment, and describe how, contrary at first to their expectations, on boiling the dibromanthraquinone with potash no change occurred, but afterwards, on using stronger potash and a higher temperature, they had the satisfaction of seeing the liquid little by little become of a violet colour; this shows the formation of alizarine. Afterwards, on acidifying this solution, the alizarine separated out in yellowish flocks. On volatalising it they got it in crystals, like those obtained from madder. On oxidising it with nitric acid, they get phthalic acid; and on precipitating it with the ordinary mordants or other metallic solutions, they get compounds exactly comparable to those from the natural product. Every trial confirms their success, so by following firmly theoretical considerations, they have been led to the discovery of the means of artificially forming this important organic colouring matter. A special interest must always attach itself to this discovery, for it is the first instance in which a natural organic colouring matter has been built up by artificial means; now the chemist can compete with Nature in its production. Although the first, it is a safe prediction that it will not long be the only one; which colouring matter will follow next it is impossible to say, but sooner or later that most interesting one, scientifically and practically, indigo will have to yield to the scientific chemist the history of its production. Returning for a moment to the percentage composition of alizarine, now that we know its constitution, its formula is established, and on comparing it ($C_{14} H_8 O_4$) with all the different formulæ which have been proposed, we see that the one advocated by Schunk was most nearly correct, in fact that it differs from it only by two atoms of hydrogen. It is not without interest to note that the next most important colouring matter in madder. Purpurine, which so pertinaciously follows alizarine, is in constitution very nearly allied to it, and is also an anthraecne derivative.

Scientifically then the artificial production of this natural product was complete, but the practical question, can it be made in the laboratory cheaper than it can be obtained from the root, had yet to be dealt with. The raw material, the anthracene, a bye-product in the manufacture of coal gas, had as yet only been obtained as a chemical curiosity; it had no market value, its cost would depend on the labour of separating it from the tar, and the amount obtainable. But with regard to the bromine necessary to form the bibromantharquinone it was different; the use of such an expensive re-agent would preclude the process becoming a manufacturing one. But could no cheaper re-agent be used in place of the bromine, and thus crown this discovery by utilising it as a manufactunrig process? It was our countryman, Mr. Perkin, who first showed how this could be done, and has since proved the very practical and important nature of his discovery by carrying it out on the manufacturing scale. The nature of Perkin's discovery was the forming in place of a bibromanthraquinone, a disulphoanthraquinone, in a word he used sulphuric acid in place of bromine, obtaining thus a sulpho acid in place of a bromine substitution compound. The properties of these sulpho acids, containing the monovalent groups $H SO_3$ which is the equivalent to the atom of bromine, is that on being boiled with an alkali they are decomposed, and a corresponding alkaline salt formed; thus the change from the anthraquinone to the alizarine was effected by boiling it with sulphuric acid. At a high temperature, it dissolves, becoming a sulpho acid $C_{14} H_6 \begin{cases}(O_2)'' \\ HSO_3 \\ HSO_3\end{cases}$ and the further changes follow, as they did with the bromine compound: the sulphuric acid boiled with potash is decomposed, and a potash salt of alizarin and potassic sulphite are formed; acid then precipitates the alizarin as a bright yellow substance. While Perkin was carrying on these researches in this country, Caro, Graebe, Liebermann, were carrying on somewhat similar ones in Germany; and in both countries have the scientific experiments developed into manufacturing industries. My knowledge extends only to the English manufactory, and if any excuse be necessary for having asked your attention to-day to this long history of a single substance, I think I must plead the existence of that manufactory as my excuse, for it is not often that purely scientific research so rapidly culminates in great practical undertakings. Already has the artificial become a most formidable opponent to the natural product; and in this struggle already begun there can be no doubt which will come off victorious. In the manufactory is rigidly carried out, the exact process I have already described to you. In tar there is about 1 per cent. of the anthracene; this, in a crude, impure state, is obtained from it by the tar-distiller, and sent by him to the colour works; here it is purified by pressure by dissolving from it many of its impurities, and lastly by volatalising it. Then comes the conversion of it into the anthraquirrone by oxidising agents, nitric or chromic acid being used. Then the formation of the sulphur compound by heating it with sulphuric acid to a temperature of about 260° C. The excess of acid present is then neutralised by the addition of lime, and the insoluble calcic sulphate is filtered off; to the filtered liquid sodic carbonate is added, and thus the calcic salt of the sulpho acid is changed into the sodic salt $C_{14} H_6 \begin{cases}(O_2)'' \\ Na\ SO_3 \\ Na\ SO_3\end{cases}$ This is afterwards heated to about 180° C. with caustic soda, thus decomposing the sulphuric acid and forming the soda salt of alizarin; and sodic sulphite; the alizarine salt so formed, remains in solution, giving to the liquid a beautiful violet colour; from this solution sulphuric acid precipitates the alizarine as an orange yellow substance. It is allowed to settle in large tanks, and then is run in the form of a yellowish sand, which contains either 10 or 15 per cent. of dry alizarine; nto barrels, and is in this form sent to the print works, and used much in the same way as the original ground madder was used.

This alizarine mud, as I have called it, containing but 10 per cent. of dry alizarine is equal in dyeing power to about eight times its weight of the best madder, and is the pure substance required for the dyeing in place of a complicated mixture containing certain constituents which have a positively injurious effect on the colours produced.

The scientific knowledge and energy which Mr. Perkin has brought to bear on the manufacture of this colouring matter, seems already to have worked wonders, the supply and demand for artificial alizarine are increasing at a most rapid rate, and yet the manufacture of it seems hardly to have commenced. The value of madder has much decreased, and in fact, judging by what occurred in the year of revolution and commercial depression, 1848, when the price of madder fell for a time to a point at which it was considered it would no longer remunerate the growers to produce it, that point has now been again reached, but certainly from very different reasons. Last year artificial alizarine, equal in value to about one-quarter of the madder imported into England, was manufactured in this country. This year the amount will be much larger. Thus is growing up a great industry, which far and wide must exercise most important effects; old and cumbersome processes must give way to better, cheaper, newer ones, and lastly thousands of acres of land in many different parts of the world will be relieved from the necessity of growing madder and be ready to receive some new crop. In this sense may the theoretical chemist be said even to have increased the boundaries of the globe.

SECTION C.—GEOLOGICAL SECTION

ADDRESS OF THE PRESIDENT, JOHN PHILLIPS, F.R.S.

MORE than half the life of an octogenarian separates us from the birthday of the British Association in Yorkshire; and few of those who then helped to inaugurate a new scientific power can be here to-day to estimate the work which it accomplished, and judge of the plans which it proposes to follow in future. Would that we might still have with us the wise leading of Harcourt, and the intrepid advocacy of Sedgwick, names dear to Geology and always to be honoured in Yorkshire!

The natural sciences in general, and Geology in particular, have derived from the British Association some at least of the advantages so boldly claimed at its origin: some impediments have been removed from their path; society looks with approbation on their efforts; their progress is hailed among national triumphs, though achieved for the most part by voluntary labour; and the results of their discoveries are written in the prosperous annals of our native industry. . . .

Turning from topics which involve industrial interests, to other lines of geological research, we remark how firmly since 1831 the great facts of rock-stratification, succession of life, earth-movement, and changes of oceanic areas have been

established and reduced to laws—laws, indeed, of phenomena at present, but gradually acquiring the character of laws of causation.

Among the important discoveries by which our knowledge of the earth's structure and history has been greatly enlarged within forty years, place must be given to the results of the labours of Sedgwick and Murchison, who established the Cambro-Silurian systems, and thus penetrated into ancient time-relics very far toward the shadowy limit of palæontological research. Stimulated by this success, the early strata of the globe have been explored with unremitting industry in every corner of the earth; and thus the classification and the nomenclature which were suggested in Wales and Cumberland are found to be applicable in Russia and India, America and Australia, so as to serve as a basis for the general scale of geological time, founded on organic remains of the successive ages.

This great principle, the gift of William Smith, is also employed with success in a fuller study of the deposits which stand among the latest in our history and involve a vast variety of phenomena, touching a long succession of life on the land, changes of depth in the sea, and alterations of climate. Among these evidences of physical revolution, which, if modern as geological events, are very ancient if estimated in centuries, the earliest monuments of men find place—not buildings, not inhabited caves or dwellings in dry earth-pits, not pottery or fabricated metal, but mere stones shaped in rude fashion to constitute apparently the one tool and one weapon with which, according to Prestwich, and Evans, and Lubbock, the poor inhabitant of northern climes had to sustain and defend his life.

Nothing in my day has had such a decided influence on the public mind in favour of geological research, nothing has so clearly brought out the purpose and scope of our science, as these two great lines of inquiry, one directed to the beginning, the other to the end of the accessible scale of earthly time; for thus has it been made clear that our purpose can be nothing less than to discover the history of the land, sea, and air, and the long sequence of life, and to marshal the results in a settled chronology—not, indeed, a scale of years to be measured by the rotations or revolutions of planets, but a series of ages slowly succeeding one another through an immensity of time.

There is no question of the truth of this history. The facts observed are found in variable combinations from time to time, and the interpretations of these facts are modified in different directions; but the facts are all natural phenomena and the interpretations are all derived from real laws of these phenomena—some certified by mathematical and mechanical research, others based on chemical discovery, others due to the scalpel of the anatomist, or the microscopic scrutiny of the botanist. The grandest of early geological phenomena have their representatives, however feeble, in the changes which are now happening around us; the forms of ancient life most surprising by their magnitude or singular adaptations can be explained by analogous though often rare and abnormal productions of to-day. Biology is the contemporary index of Palæontology, just as the events of the nineteenth century furnish explanations of the course of human history in the older times. . . .

During the long course of geological time the climates of the earth have changed. In many regions evidence of such change is furnished by the forms of contemporary life. Warm climates have had their influence on the land, and favoured the growth of abundant vegetations as far north as within the arctic circle; the sea has nourished reef-making corals in northern Europe during Palæozoic and Mesozoic ages; crocodiles and turtles were swimming round the coasts of Britain, among islands clothed with Zamiæ and haunted by marsupial quadrupeds. How have we lost this primæval warmth? Does the earth contribute less heat from its interior stores? does the atmosphere obstruct more of the solar rays or permit more free radiation from the land and sea? has the sun lost through immensity of time a sensible portion of its beneficent influence? or, finally, is it only a question of the elevation of mountains, the oceanic currents, and the distribution of land and sea?

The problems thus suggested are not of easy solution, though in each branch of the subject some real progress is made. The globe is slowly changing its dimensions by cooling; thus inequalities and movements of magnitude have arisen and are still in progress on its surface: the effect of internal pressure, when not resulting in mass-movement, is expressed in the molecular action of heat which Mallet applies to the theory of volcanoes. The sun has no recuperative auxiliary known to Thomson for replacing his decaying radiation; the earth, under his influence, as was shown by Herschel and Adhemar, is subject to periods of greater and less warmth, alternately in the two hemispheres and generally over the whole surface; and finally, as Hopkins has shown, by change of local physical conditions the climate of northern zones might be greatly cooled in some regions and greatly warmed in others.

One is almost frozen to silence in presence of the vast sheets of ice which some of my friends (followers of Agassiz) believe themselves to have traced over the mountains and vales of a great part of the United Kingdom, as well as over the kindred regions of Scandinavia. One shudders at the thought of the innumerable icebergs with their loads of rock, which floated in the once deeper North Sea, and above the hills of the three Ridings of Yorkshire, and lifted countless blocks of Silurian stone from lower levels, to rest on the precipitous limestones round the sources of the Ribble.

Those who, with Professor Ramsay, adopt the glacial hypothesis in its full extent, and are familiar with the descent of ice in Alpine valleys where it grinds and polishes the hardest rocks, and winds like a slow river round projecting cliffs, are easily conducted to the further thought that such valleys have been excavated by such ice-rubbers, and that even great lakes on the course of the rivers have been dug out by ancient glaciers which once extended far beyond their actual limits. That they did so extend is in several instances well ascertained and proved; that they did in the manner suggested plough out the valleys and lakes is a proposition which cannot be accepted until we possess more knowledge than has yet been attained regarding the resistance offered by ice to a crushing force, its tensile strength, the measure of its resistance to shearing, and other data required for a just estimate of the problem. At present it would appear that under a column of its own substance 1000 ft. high, ice would not retain its solidity; if so, it could not propagate a greater pressure in any direction. This question of the excavating effect of glaciers is distinctly a mechanical problem, requiring a knowledge of certain data; and till these are supplied, calculations and conjectures are equally vain.

A distinguishing feature of modern geology is the greater development of the doctrine that the earth contains in its burial-vaults, in chronological order, forms of life characteristic of the several successive periods when stratified rocks were deposited in the sea. This idea has been so thoroughly worked upon in all countries, that we are warranted to believe in something like one universal order of appearance in time, not only of large groups but even of many genera and species. The Trilobitic ages, the Ammonitic, Megalosaurian, and Palæotherian periods are familiar to every geologist. What closed the career of the several races of plants and animals on the land and in the sea, is a question easily answered for particular parts of the earth's surface by reference to "physical change;" for this is a main cause of the presence or absence, and in general of the unequal distribution of life. But what brought the succession of different races in something like a constant order, not in one tract only, but one may say generally in oceanic areas, over a large portion of the globe?

Life unfolds itself in every living thing, from an obscure, often undistinguishable cell germ, in which resides a potential of both physical and organic change—a change which, whether continual or interrupted, gradual or critical, culminates in the production of similar germs, capable under favourable conditions of assuming the energy of life.

How true to their prototypes are all the forms with which we are familiar, how correctly they follow the family pattern for centuries, and even thousands of years, is known to all students of ancient art and explorers of ancient catacombs. But much more than this is known. Very small differences separate the elephant of India from the mammoth of Yorkshire, the *Waldheimia* of the Australian shore from the *Terebratula* of the Cotswold oolite, the dragon-fly of our rivers from the *Libellula* of the Lias, and even the *Rhynchonellæ* and *Lingulæ* of the modern sea from the old species which swarm in the Palæozoic rocks.

But concurrently with this apparent perpetuity of similar forms and ways of life, another general idea comes into notice. No two plants are more than alike; no two men have more than the family resemblance; the offspring is not in all respects an exact copy of the parent. A general reference to some earlier type, accompanied by special diversity in every case ("descent with modification"), is recognised in the case of every living being.

Similitude, not indentity, is the effect of natural agencies in the continuation of life-forms, the small differences from identity being due to limited physical conditions, in harmony with the general law that organic structures are adapted to the exigencies of being. Moreover, the structures are adaptable to new conditions; if the conditions change, the structure changes also, but not suddenly; the plant or animal may survive in presence of slowly altered circumstances, but must perish under critical inversions. These adaptations, so necessary to the preservation of a race, are they restricted within narrow limits? or is it possible that in course of long-enduring time, step by step and grain by grain, one form of life can be changed and has been changed to another, and adapted to fulfil quite different functions? It is thus that the innumerable forms of plants and animals have been "developed" in the course of ages upon ages from a few original types?

This question of development might be safely left to the prudent researches of Physiology and Anatomy, were it not the case that Palæontology furnishes a vast range of evidence on the real succession in time of organic structures, which on the whole indicate more and more variety and adaptation, and in certain aspects a growing advance in the energies of life. Thus at first only invertebrate animals appear in the catalogues of the inhabitants of the sea, then fishes are added, and reptiles and the higher vertebrata succeed; man comes at last, to contemplate and in some degree to govern the whole.

The various hypothetical threads by which many good naturalists hope to unite the countless facts of biological change into an harmonious system have culminated in Darwinism, which takes for its basis the facts already stated, and proposes to explain the analogies of organic structure by reference to a common origin, and their differences to small, mostly congenital, modifications which are integrated in particular directions by external physical conditions, involving a "struggle for existence." Geology is interested in the question of development, and in the particular exposition of it by the great naturalist whose name it bears, because it alone possesses the history of the development *in time*, and it is to inconceivably long periods of time, and to the accumulated effect of small but almost infinitely numerous changes in certain directions, that the full effect of the transformations is attributed.

For us, therefore, at present it is to collect with fidelity the evidence which our researches must certainly yield, to trace the relation of forms to time generally and physical conditions locally, to determine the life-periods of species, genera, and families in different regions, to consider the cases of temporary interruption and occasional recurrence of races, and how far by uniting the results obtained in different regions the alleged "imperfection of the geological record" can be remedied.

The share which the British Association has taken in this great work of actually reconstructing the broken forms of ancient life, of repeopling the old land and older sea, of mentally reviving, one may almost say, the long-forgotten past, is considerable, and might with advantage be increased. We ask, and wisely, from time to time, for the combined labour of naturalists and geologists in the preparation of reports on particular classes or families of fossil plants and animals, their true structure and affinities, and their distribution in geological time and geographical space. Some examples of this useful work will, I hope, be presented to this meeting. Thus have we obtained the aid of Agassiz and Owen, and have welcomed the labours of Forbes and Morris, and Lycett, and Huxley, of Dawkins and Egerton, of Davidson, Duncan, and Wright, of Williamson and Carruthers and Woodward, and many other eminent persons, whose valuable results have for the most part appeared in other volumes than our own.

Among these volumes let me in a special manner recall to your attention the priceless gift to Geology which is annually offered by the Palæontographical Society, a gift which might become even richer than it is, if the literary and scientific part of our community were fortunate enough to know what a perpetual treasure they might possess in return for a small annual tribute. The excellent example set and the good work recorded in the Memoirs of the Society referred to have not been without influence on foreign men of science. We shall soon have such Memoirs from France and Italy, Switzerland and Germany, America and Australia; and I trust the effect of such generous rivalry will be to maintain and increase the spirit of learned research and of original observation which it is our privilege and our duty to foster, to stimulate, and to combine.

On all the matters, indeed, which have now been brought, to your thoughts the one duty of geologists is to collect more and more accurate information; the one fault to be avoided is the supposition that our work in any department is complete. We should speak modestly of what has been done; for we have completed nothing, except the extinction of a crowd of errors, and the discovery of right methods of proceeding toward the acquisition of truth. We may speak hopefully of what is to be accomplished; for the right road is before us. We have taken some steps along it; others will go beyond us and stand on higher levels. But it will be long before anyone can reach the height from which he may be able to survey the whole field of research and collect the results of ages of labour.

SECTION D.—BIOLOGY

OPENING ADDRESS BY THE PRESIDENT, PROF. ALLMAN

The present Aspects of Biology and the Method of Biological Study

FOR some years it has been the practice at the meetings of this Association for the special presidents to open the work of their respective sections with an address which is supposed to differ, in the greater generality of its subject, from the ordinary communications to the sections. Finding that during the present meeting this duty would devolve on myself, I thought over the available topics, and concluded that a few words on the present aspect of Biology and the method of Biological Study would best satisfy the conditions imposed.

I shall endeavour to be as little technical as my subject will allow, and though I know that there are here present many to whom I cannot expect to convey any truths with which they are not already familiar, yet in an address of this kind the speaker has no right to take for granted any large amount of scientific knowledge in his audience. Indeed, one of the chief advantages which result from these meetings of the British Association consists in the stimulus they give to inquiry—in the opportunity they afford to many of becoming acquainted for the first time with the established truths of Science, and the initiation among them of new lines of thought.

And this is undoubtedly no small gain; for how many are there who, though they may have reaped all the advantages which our established educational systems can bestow, are yet sadly deficient in a knowledge of the world of life which surrounds them. It is a fair and wonderful world, this on which we have our dwelling-place, and yet how many wander over it unheedingly? by how many have its lessons of wisdom never been read? how many have never spared a thought on the beauty of its forms, the harmony of its relations, the deep meaning of its laws?

And with all this there is assuredly implanted in man an undying love of such knowledge. From his unshaken faith in causation he yearns to deduce the unknown from the known, to look beyond what is at hand and obvious to what is remote and unseen.

Conception of Biology and Function of the Scientific Method

Under the head of Biology are included all those departments of scientific research which have as their object the investigation of the living beings—the plants and the animals—which tenant the surface of our earth, or have tenanted it in past time.

It admits of being divided under two grand heads: Morphology, which treats of Form, and Physiology, which treats of Function; and besides these there are certain departments of Biological study to which both Morphology and Physiology contribute, such as Classification, Distribution, and that department of research which is concerned with the origin and causes of living and extinct forms.

By the aid of observation and experiment we obtain the elements which are to be combined and developed into a science of living beings, and it is the function of the scientific method to indicate the mode in which the combinations are to be effected, and the path which the development must pursue. Without it the results gained would be but a confused assemblage of isolated facts and disconnected phenomena; but aided by a philosophic method, the observed facts become scientific propositions, what was apparently insignificant becomes full of meaning, and we get glimpses of the consummate laws which govern the whole.

Importance of Anatomy

The first step in our morphological study of human beings is to obtain an accurate and adequate knowledge of the forms of the individual objects which present themselves to us in our contemplation of the animal and vegetable kingdoms. For such knowledge, however, much more is needed than an acquaintance with their external figure. We must subject them to a searching scrutiny; we must make ourselves familiar with their anatomy, which involves not only a knowledge of the forms and disposition of their organs, internal as well as external, but of their histology, or the microscopic structure of the tissues of which these organs are composed. Histology is nothing more than Anatomy carried to its extreme term, to that point where it meets with the Morphological Unit, the ultimate element of form, and the simplest combinations of this out of which all the organs in the living body are built up.

Among the higher animals Anatomy, in the ordinary sense of the word, is sufficiently distinct from Histology to admit of separate study; but in the lower animals and in plants the two become confounded at so many points as to render their separate study often impracticable.

Now the great prominence given to Anatomy is one of the points which most eminently distinguish the modern schools of Biology.

Development

Another order of morphological facts of scarcely less importance than those obtained from anatomical study is that derived from the changes of form which the individual experiences during the course of its life. We know that every organised being commences existence as a simple sphere of protoplasm, and that from this condition of extreme generalisation all but the very lowest pass through phases of higher and higher specialisation acquiring new parts and differentiating new tissues. The sum of these changes constitutes the development of the organisms, and no series of facts is more full of significance in its bearing on Biological Science than that which is derived from the philosophical study of Development.

Classification an Expression of Affinities

Hitherto we have been considering the individual organism without any direct reference to others. But the requirements of the biological method can be satisfied only by a comparison of the various organisms one with the other. Now the grounds of such comparison may be various, but what we are at present concerned with will be found in anatomical structure and in developmental changes; and in each of these directions facts of the highest order and of great significance become apparent.

By a carefully regulated comparison of one organism with another, we discover the resemblances as well as the differences between them. If these resemblances be strong, and occur in important points of structure or development, we assert that there is an affinity between the compared organisms, and we assume that the closeness of the affinity varies directly with the closeness of the resemblance.

It is on the determination of these affinities that all philosophic classification of animals and plants must be based. A philosophical classification of organised beings aims at being a succinct statement of the affinities between the objects so classified, these affinities being at the same time so set forth as to have their various degrees of closeness and remoteness indicated in the classification.

Affinities have long been recognised as the grounds of a natural biological classification, but it is only quite lately that a new significance has been given to them by the assumption that they may indicate something more than simple agreement with a common plan—that they may be derived by inheritance from a common ancestral form, and that they therefore afford evidence of a true blood relationship between the organisms presenting them.

The recognition of this relationship is the basis of what is known as the Descent Theory. No one doubts that the resemblances we notice among the members of such small groups as those we name species are derived by inheritance from a common ancestor, and the Descent Theory is simply the extension to the larger groups of this same idea of relationship.

If this be a true principle, then biological classification becomes an exposition of family relationship—a genealogical tree in which the stem and branches indicate various degrees of relationship and direct and collateral lines of descent. It is this conception which takes classification out of the domain of the purely Morphological.

Affinity determined by the Study of Anatomy and Development

From what has just been said it follows that it is mainly by a comparison of organisms in their anatomical and developmental characters that their affinities are discoverable. The structure of an organism will in by far the greater number of cases be sufficient to indicate its true affinity, but it sometimes happens that certain members of a group depart in their structure so widely from the characters of the type to which they belong, that without some other evidence of their affinities no one would think of assigning them to it. This evidence is afforded by development.

An example or two will serve to make the subject clear, and we shall first take one from a case where, without a knowledge of anatomical structure, we should easily go astray in our attempts to assign to the forms under examination their true place in the classification.

If we search our coasts at low water we shall be sure to meet with certain plant-like animals spreading over the rocks or rooted to the fronds of sea-weeds, all of which present so close a resemblance to one another as to have led to their being brought together into a single group to which, under the name of "Polypes," a definite place was assigned in the classification of the animal kingdom.

They are all composite animals consisting of an association of buds or zooids, which remain organically united to one another, and give to the whole assemblage the appearance in many cases of a little branching tree. Every bud carries a delicate transparent cup of chitine within which is contained the principal part of the animal, and from which this has the power of spontaneously protruding itself; and when thus protruded it will be seen to present a beautiful crown of tentacles surrounding a mouth through which food is taken into a stomach. As long as no danger threatens, the little animal will continue displayed with its beautiful corona of tentacles expanded; but touch it ever so lightly, and it will instantly close up its tentacles, retract its whole body, and take refuge in the recesses of the protecting cup.

So far then there is a complete agreement between the animals which have been thus associated under the designation of Polypes, and in all that concerns their external form no one point can be adduced in opposition to the justice of this association. When, however, we pass below the surface and bring the microscope and dissecting needle to bear on their internal organisation, we find that among the animals thus formed so apparently alike, we have two totally distinct types of structure; that while in one the mouth leads into a simple excavation of the body on which devolves the whole of the functions which represent digestion, in the other there is a complete alimentary tract entirely shut off from the proper cavity of the body and consisting of distinctly differentiated œsophagus, stomach, and intestine; while in the one the muscular system consists of an indistinct layer of fibres intimately united in its whole extent with the body walls, in the other there are distinctly differentiated free bundles of muscles for the purpose of effecting special motions in the economy of the animal; while in the one no differentiated nervous system can be detected, in the other there is a distinct nervous ganglion with nervous filaments. In fact the two forms are shown by a study of their anatomical structure to belong to two entirely different primary divisions of the animal kingdom; for while the one has a close affinity with the little fresh-water Hydra, and is therefore referred to the Hydroida among the sub-kingdom Cœlenterata, the other is referable to the group of the Polyzoa; it has its immediate affinities with the Ascidians, and belongs to the sub-kingdom of the Mollusca.

We shall next take an example in which the study of development rather than of anatomy affords the clue to the true affinities of the organism.

Attached to the abdomen of various crabs may often be seen certain soft fleshy sacs to which the name of *Sacculina* has been given. They hold their place by means of a branching root-like extension which penetrates the abdomen of the crab and winds itself round its intestine or dives into its liver, within which its fibres ramify like the roots of a tree.

Now the question at once presents itself: what position in the animal kingdom are we to assign to this immoveably rooted sac destitute of mouth and of almost every other organ with which we are in the habit of associating the structure of an animal?

Anatomy will here be powerless in helping us to arrive at a

conclusion, for the dissecting knife shows us little more than a closed sac filled with eggs and fixed by its tenacious roots in the viscera of its victim. Let us see, however, what we learn from development. If some of the eggs with which the Sacculina is filled be placed in conditions suited to their development, they give origin to a form as different as can well be imagined from the sacculina. It is an active, somewhat oval-shaped little creature, covered with a broad dorsal shield or carapace, and furnished with two pairs of strong swimming feet which carry long bristles, and also with a pair of anterior limbs or antennæ. It is, in fact, identical with a form known to zoologists by the name "Nauplius," and which has been proved to be one of the young states of the Barnacle and of other lower crustacea; while even some of the higher crustacea have been observed to pass through a similar stage.

After a short time the Nauplius of our Sacculina changes its form; the carapace folds down on each side and assumes the shape of a little bivalve shell; while six new pairs of swimming feet are developed. The little animal continues its active natatory life, and in this stage it is again identical in all essential points with one of the young stages of the Barnacle.

In the meantime a remarkable change takes place in the two antennæ; they become curiously branched and converted into prehensile organs. The young Sacculina now seeks the crab on which it is to spend parasitically the rest of its life; it loses its bivalve shell, the prehensile antennæ takes hold of its victim, penetrates the soft skin of its abdomen in order to seek within it the nutriment with which it can be there so plentifully supplied, locomotion is gone for ever, and the active and symmetrical Nauplius becomes converted into the inert and shapeless Sacculina.

The nearest affinities of Sacculina are thus undoubtedly with the Barnacles, which have been proved both on anatomical and developmental grounds to belong to the great division of the Crustacea.

A Philosophical Classification cannot form a single Rectilineal Series

A comparison of animals with one another having thus resulted in establishing their affinities, we may arrange them into groups, some more nearly, others more remotely related to one another. The various degrees and directions of affinity will be expressed in every philosophical arrangement, and as these affinities extend in various directions, it becomes at once apparent that no arrangement of the animal or vegetable kingdom in a straight line ascending like the steps of a ladder from lower to higher forms, can give a true idea of the relations of living beings to one another. These relations, on the contrary, can be expressed only by a ramified and complex figure which we have already compared to that of a genealogical tree.

Homology

In the comparison of organised beings with one another, certain relations of great interest and significance become apparent between various organs. These are known by the name of Homologies, and organs are said to be homologous with one another when they can be proved to be constructed on the same fundamental plan, no matter how different they may be in form and in the functions which they may be destined to execute. Organs not constructed on the same fundamental plan may yet execute similar functions, and then, whether they do or do not resemble one another in form, they are said to be merely analogous; and some of the most important steps in modern Biology have resulted from attention to the distinction between Homology and Analogy, a distinction which was entirely disregarded by the earlier schools.

The nature of Homology and its distinction from Analogy will be best understood by a few examples.

Compare the wing of a bird with that of an insect; there is a resemblance between them in external form; there is also an identity of function, both organs being constructed for the purpose of flight, and yet they are in no respect homologous, for they are formed on two distinct plans which have nothing whatever in common. The relation between them is that simply of analogy.

On the other hand, no finer illustrations of Homology can be adduced than those which are afforded by a comparison with one another of the anterior limbs among the various members of the vertebrata. Let us compare, for example, the bird's wing with the anterior limb of man. Here we have two organs between which the ordinary observer would fail to recognise any resemblance—organs, too, whose functions are entirely different, one being formed for prehension and the other for flight. When, however, they are compared in the light which a philosophic anatomy is capable of throwing on them, we find, between the two, a parallelism which points to one fundamental type on which they are both constructed.

There is first the shoulder-girdle, or system of bones by which, in each case, the limb is connected with the rest of the skeleton. Now this part of the skeleton in man is very different in form from the same part in the bird, and yet a critical comparison of the two shows us that the difference mainly consists in the fact that the coracoid which in man is a mere process of the scapula, is in the bird developed as an independent bone; and in the further fact that the two clavicles in man are, in the bird, united into a single V-shaped bone or "furcula." Then, if we can compare the arm, fore-arm, wrist, and hand in the human skeleton with the various parts which follow one another in the same order in the skeleton of the bird's wing, we shall find between the two series a correspondence which the adaptations to special functions may in some regions mask, but never to such an extent as to render the fundamental unity of plan difficult of detection by the method of the higher anatomy. As far as regards the arm and fore-arm, these in the bird are nearly repetitions of their condition in the human skeleton; but the parts which follow appear at first sight so different as to have but little relation with one another, and yet a common line can be traced with great distinctness through the two. Thus the wrist is present in the bird's wing as well as in the anterior limb of man, but while in man it is composed of eight small irregularly-shaped bones arranged in two rows, in the wing it has become greatly modified, the eight bones being reduced to two. Lastly, the hand is also represented in the wing, where it constitutes a very important part of the organ of flight, but where it has undergone such great modification as to be recognisable only after a critical comparison; for the five metacarpal bones of the human hand are reduced to two consolidated with one another at their proximal and distal ends; and then the five fingers of the hand are reduced in the wing to three, which represent the middle finger, fore-finger, and thumb. The fore-finger in the bird consists of only one phalanx, the middle of two, and the thumb forms a small stiletto-like bone springing from the proximal end of the united metacarpals.

In the case now adduced we have an example of the way in which the same organ in two different animals may become very differently modified in form, so as to fit it for the performance of two entirely different functions, and yet retain sufficient conformity to a common plan to indicate a fundamental unity of structure.

Let us take another example, and this I shall adduce from the vegetable kingdom, which is full of beautiful instances of the relations with which we are now occupied.

There are the parts known as tendrils, thread-like organs, usually rolling themselves into spirals, and destined, by twining round some fixed support, to sustain climbing plants in their efforts to raise themselves from the ground. We shall take two examples of these beautiful appendages, and endeavour to determine their homological significance.

There is the genus *Smilax*, one species of which adorns the hedges of the south of Europe, where it takes the place of the Bryony and *Tamus* of our English country lanes. From the point where the stalks of its heart-shaped leaves spring from the stem, there is given off a pair of tendrils by means of which the Smilax clings to the surrounding vegetation in an inextricable entanglement of branches and foliage.

With the tendrils of the Smilax let us compare those of the *Lathyrus aphaca*, a little vetch occasionally met with in waste places and the margins of corn-fields. The leaves are represented by arrow-shaped leaf-like appendages, which are placed opposite to one another in pairs upon the stem, but instead of each of these carrying two tendrils at its origin like the leaves of the Smilax, a single tendril springs from the middle point between each pair.

The tendrils in the two cases, though similar in appearance and in function, differ thus in number and arrangement, and the questions occur: are they homologous with one another, or are they only analogous? and if they be only analogous, can we trace between them and any other organ homologous relations?

To enable us to decide on this point, we must bear in mind that a leaf when typically developed consists of three portions, the lamina or blade, the petiole or leaf-stalk, and a pair of

foliaceous appendages or stipules, which are placed at the base of the leaf-stalk. Now this typical leaf affords the key to the homologies of the tendrils in the two cases under examination.

Take the Smilax: in this case there are no stipules of the ordinary form, but the two tendrils hold exactly the position of the stipules in our type-leaf, and must be regarded as representing them. We have only to imagine these stipules so modified in their form as to become reduced to two long spiral threads, and we shall at once have the tendrils of the Smilax; on the other hand let the stipules in our type remain as leaf-like organs, and let the rest of the leaf—the lamina and petiole—lose its normal character, and become changed into a spiral thread, and we shall then have the stipules of our type-leaf retained in the two opposite leaf-like organs of the Lathyrus, while the remainder of the type-leaf will present itself in the condition of the Lathyrus tendril which springs from the central point between them.

The tendrils of the Smilax and of the *Lathyrus aphaca* are thus not homologous with one another, but only analogous, while those of the Smilax are homologous with a pair of stipules and those of the Lathyrus homologous with the lamina and petrile of a leaf.

Besides the homology discoverable between the organs of different animals and plants, a similar relation can be traced between organs in the same animal or plant; as, for example, that between the different segments of the vertebral column, which can be shown to repeat one another homologically; and that between the parts composing the various verticils of the flower and the leaves in the plant.

The existence of homological relations such as have been just illustrated admits of an easy explanation by the application of the doctrine of descent, according to which the two organs compared would originate from a common ancestral form. In accordance with this hypothesis, homology would mean an identity of genesis in two organs, as analogy would mean an identity of function.

Distribution and Evolution

Another very important department of biological science is that of the Distribution of organised beings. This may be either Distribution in Space, Geographical Distribution: or Distribution in Time, Palæontological Distribution. Both of these have of late years acquired increased significance, for we have begun to get more distinct glimpses of the laws by which they are controlled, of the origin of Faunas and Floras, and of the causes which regulate the sequence of life upon the earth. Time, however, will not allow me to enter upon this subject as fully as its interest and importance would deserve, and a few words on Palæontological Distribution is all that I can now venture on.

The distribution of organised beings in time has lately come before us in a new light by the application to it of the hypothesis of evolution. According to this hypothesis, the higher groups of organised beings now existing on the earth's surface have come down to us with gradually increasing complexity of structure by continuous descent from forms of extreme simplicity which constituted the earliest life of our planet.

In almost every group of the animal kingdom the members which compose it admit of being arranged in a continuous series passing down from more specialised, or higher, to more generalised or lower forms; and if we have any record of extinct members of the group, the series may be carried on through these. Now while the descent hypothesis obliges us to regard the various terms of the series as descended from one another, the most generalised forms will be found among the extinct ones, and the further back in time we go the simpler do the forms become.

By a comparison of the forms so arranged we obtain as it were the law of the series, and can thus form a conception of the missing terms and continue the series backwards through time, even where no record of the lost forms can be found, until from simpler to still simpler terms we at last arrive at the conception of a term so generalised that we may regard it as the primordial stock, the ancestral form from which all the others have been derived by descent.

This root form is thus not actually observed, but is rather obtained by a process of deduction, and is therefore hypothetical. We shall strengthen, however, its claims to acceptance by the application of another principle. The study of embryology shows that the higher animals, in the course of their development, pass through transitory phases which have much in common with the permanent condition of lower members of the type to which they belong, and therefore with its extinct representatives. We are thus enabled to lay down the further principle that the individual, in the course of its own development from the egg to the fully formed state, recapitulates within that short period of time the various forms which its ancestry presented in consecutive epochs of the world's history; so that if we knew all the stages of its individual development, we should have a long line of its descent. Through the hypothesis of evolution, palæontology and embryology are thus brought into mutual bearing on one another.

Let us take an example in which these two principles seem to be illustrated. In rocks of the Silurian age there exist in great profusion the remarkable fossils known as graptolites. These consist of a series of little cups or cells arranged along the sides of a common tube, and the whole fossil presents so close a resemblance to one of the Sertularian hydroids which inhabit the waters of our present seas as to justify the suspicion that the graptolites constitute an ancient and long since extinct group of the Hydroida. It is not, however, with the proper cells or hydrothecæ of the Sertularians that the cells of the graptolite most closely agree, but rather with the little receptacles which in certain Sertularinæ belonging to the family of the Plumularida we find associated with the hydrothecæ, and which are known as "Nematophores," a comparison of structure then shows that the graptolites may with considerable probability be regarded as representing a Plumularia in which the hydrothecæ had never been developed and in which their place had been taken by the nematophores.

Now it can be shown that the nematophores of the living Plumularida are filled with masses of protoplasm which have the power of throwing out pseudopodia, or long processes of their substance, and that they thus resemble the Rhizopoda, whose soft parts consist entirely of a similar protoplasm and which stand among the Protozoa or lowest group of the animal kingdom. If we suppose the hydrotheca suppressed in a plumularian, we should thus nearly convert it into a colony of Rhizopoda, from which it would differ only in the somewhat higher mophological differentiation of its cœnosarc or common living bond by which the individuals of the colony are organically connected. And just such a colony would, under this view, a graptolite be, waiting only for the development of hydrotheca to raise it into the condition of a plumularian.

Bringing now the evolution hypothesis to bear upon the question, it would follow that the graptolite may be viewed as an ancestral form of the Sertularian hydroids, a form having the most intimate relations with the Rhizopoda; that hydranths and hydrothecæ became developed in its descendants; and that the rhizopodal graptolite became thus converted in the lapse of ages into the hydroidal Sertularian.

This hypothesis would be strengthened if we found it agreeing with the phenomena of individual development. Now such Plumularida as have been followed in their development from the egg to the adult state do actually present well-developed nematophores before they show a trace of hydrothecæ, thus passing in the course of their embryological development through the condition of a graptolite, and recapitulating within a few days stages which it took incalculable ages to bring about in the palæontological development of the tribe.

I have thus dwelt at some length on the doctrine of evolution because it has given a new direction to biological study and must powerfully influence all future researches. Evolution is the highest expression of the fundamental principles established by Mr. Darwin, and depends on the two admitted faculties of living beings—*heredity*, or the transmission of characters from the parent to the offspring; and *adaptivity*, or the capacity of having these characters more or less modified in the offspring by external agencies, or it may be by spontaneous tendency to variation.

The hypothesis of evolution may not, it is true, be yet established on so sure a basis as to command instantaneous acceptance, and for a generalisation of such vast significance no one can be blamed for demanding for it a broad and indisputable foundation of facts. Whether, however, we do or do not accept it as firmly established, it is at all events certain that it embraces a greater number of phenomena and suggests a more satisfactory explanation of them than any other hypothesis which has yet been proposed.

With all our admiration, however, for the doctrine of Evolution as one of the most fertile and comprehensive of philosophic hypotheses, we cannot shut our eyes to the difficulties which lie

n the way of accepting it to the full extent which has been sometimes claimed for it. It must be borne in mind that though among some of the higher vertebrata we can trace back for some distance in geological time a continuous series of forms which may safely be regarded as derived from one another by gradual modification—as has been done, for example, so successfully by Prof. Huxley in the case of the horse—yet the instances are very few in which such a sequence has been actually established; while the first appearance in the earth's crust of the various classes presents itself in forms which by no means belong to the lowest or most generalised of their living representatives. On this last fact, however, I do not lay much stress, for it will admit of explanation by referring it to the deficiency of the geological record, and then demanding a lapse of time—of enormous length, it is true—during which the necessary modifications would be in progress before the earliest phase of which we have any knowledge could have been reached.

Again, we must not lose sight of the hypothetical nature of those primordial forms in which we regard the branches of our genealogical tree as taking their origin; and while the doctrine of the recapitulation of ancestral forms has much probability, and harmonises with the other aspects of the Evolution doctrine into a beautifully symmetrical system, it is one for which a sufficient number of actually observed facts has not yet been adduced to remove it altogether from the region of hypothesis.

Even the case of the graptolites already adduced is an illustration rather than a proof, for the difficulty of determining the true nature of such obscure fossils is so great that we may be altogether mistaken in our views of their structure and affinities.

To me, however, one of the chief difficulties in the way of the doctrine of Evolution, when carried out to the extreme length for which some of its advocates contend, appears to be the unbroken continuity of inherited life which it necessarily requires through a period of time whose vastness is such that the mind of man is utterly incapable of comprehending it. Vast periods, it is true, are necessary in order to render the phenomena of Evolution possible; but the vastness which the antiquity of life, as shown by its remains in the oldest fossiliferous strata, requires us to give to these periods may be even greater than is compatible with continuity.

We have no reason to suppose that the reproductive faculty in organised beings is endowed with unlimited power of extension, and yet to go no farther back than the Silurian period—though the seas which bore the Eozoon were probably as far anterior to those of the Silurian as these are anterior to our own—the hypothesis of Evolution requires that in that same Silurian period the ancestors of the present living forms must have existed, and that their life had continued by inheritance through all the ramifications of a single genealogical tree down to our own time; the branches of the tree, it is true, here and there falling away, with the extinction of whole genera and families and tribes, but still some always remaining to carry on the life of the base through a period of time to all intents and purposes infinite. It is true that in a few cases a continuous series of forms regularly passing from lower to higher degrees of specialisation, and very probably connected to another by direct descent, may be followed through long geological periods, as for example, the graduated series already alluded to, which may be traced between certain mammals of the Eocene and others living in our own time, as well as the very low forms which have come down to us apparently unmodified from the epoch of the Chalk. But incalculably great as are these periods, they are but as the swing of the pendulum in the Millennium, when compared to the time which has elapsed since the first animalisation of our globe.

Is the faculty of reproduction so wonderfully tenacious as all this, that through periods of inconceivable duration, and exposed to influences the most intense and the most varied, it has still come down to us in an unbroken stream? Have the strongest which had survived in the struggle for existence necessarily handed down to the strongest which should follow them the power of continuing as a perpetual heirloom the life which they had themselves inherited? Or have there been many total extinctions and many renewals of life—a succession of genealogical trees, the earlier ones becoming old and decayed, and dying out, and their place taken by new ones which have no kinship with the others? Or, finally, is the doctrine of Evolution only a working hypothesis which, like an algebraic fiction, may yet be of inestimable value as an instrument of research? For as the higher calculus becomes to the physical inquirer a power by which he unfolds the laws of the inorganic world, so may the hypothesis of Evolution, though only a hypothesis, furnish the biologist with a key to the order and hidden forces of the world of life. And what Leibnitz and Newton and Hamilton have been to the physicist, is it not that which Darwin has been to the biologist?

But even accepting as a great truth the doctrine of Evolution, let us not attribute to it more than it can justly claim. No valid evidence has yet been adduced to lead us to believe that inorganic matter has become transformed into living, otherwise than through the agency of a pre-existing organism, and there remains a residual phenomenon still entirely unaccounted for. No physical hypothesis founded on any indisputable fact has yet explained the origin of the primordial protoplasm, and, above all, of its marvellous properties which render Evolution possible.

Accepting, then, the doctrine of Evolution in all freedom and in all its legitimate consequences, there remains, I say, a great residuum unexplained by physical theories. Natural Selection, the Struggle for Existence, the Survival of the Fittest, will explain much, but they will not explain all. They may offer a beautiful and convincing theory of the present order and fitness of the organic universe, as the laws of attraction do of the inorganic, but the properties with which the primordial protoplasm is endowed—its heredity and its adaptivity—remain unexplained by them, for these properties are their cause and not their effect.

For the cause of this cause we have sought in vain among the physical forces which surround us, until we are at last compelled to rest upon an independent volition, a far-seeing intelligent design. Science may yet discover even among the laws of Physics the cause it looks for; it may be that even now we have glimpses of it; that those forces among which recent physical research has demonstrated so grand a unity—Light, Heat, Electricity, Magnetism—when manifesting themselves through the organising protoplasm, become converted into the phenomena of life, and that the poet has unconsciously enunciated a great scientific truth when he tells us of

> "Gay lizards glittering on the walls
> Of ruined shrines, busy and bright
> As though they were *alive with light*."

But all this is only carrying us one step back in the grand generalisation. All science is but the intercalation of causes, each more comprehensive than that which it endeavours to explain, between the great primal cause and the ultimate effect.

I have thus endeavoured to sketch for you in a few broad outlines the leading aspects of biological science, and to indicate the directions which biological studies must take. Our science is one of grand and solemn import, for it embraces man himself and is the exponent of the laws which he must obey. Its subject is vast, for it is Life, and Life stretches back into the illimitable past, and forward into the illimitable future. Life, too, is everywhere. Over all this wide earth of ours, from the equator to the poles, there is scarcely a spot which has not its animal or its vegetable denizens—dwellers on the mountain and on the plain, in the lake and on the prairie, in the arid desert and the swampy fen; from the tropical forest with its strange forms and gorgeous colours, and myriad voices, to the ice-fields of polar latitudes and those silent seas which lie beneath them, where living things unknown to warmer climes congregate in unimaginable multitudes. There is life all over the solid earth; there is life throughout the vast ocean, from its surface down to its great depths, deeper still than the lead of sounding-line has reached.

And it is with these living hosts, unbounded in their variety, infinite in their numbers, that the student of biology must make himself acquainted. It is no light task which lies before him —no mere pastime on which he may enter with trivial purpose, as though it were but the amusement of an hour; it is a great and solemn mission to which he must devote himself with earnest mind and with loving heart, remembering the noble words of Bacon:—

"Knowledge is not a couch whereon to rest a searching and restless spirit; nor a terrace for a wandering and variable mind to walk up and down with a fair prospect; nor a tower of state for a proud mind to raise itself upon; nor a fort or commanding-ground for strife and contention; nor a shop for profit and sale; but a rich storehouse for the glory of the Creator, and the relief of man's estate."

SECTION G.—MECHANICAL SCIENCE

OPENING ADDRESS BY THE PRESIDENT, W. H. BARLOW, C.E., F.R.S.

In the observations which I have to address to you I shall not attempt a general survey of a subject so vast and so varied as the manufactures of this country, nor shall I attempt to describe the many new and beautiful inventions and mechanical appliances which form a distinguishing feature of the age in which we live; but I shall endeavour to draw your attention to one of the new materials, namely *modern steel*—a material which, though of comparatively recent origin, has already become an important industry, and whose influence in the future seems destined to vie in importance with that resulting from the introduction of iron.

I have used the term "modern steel," because, although the great movement in simplifying and cheapening the process of producing steel is necessarily associated with the name of Mr. Bessemer, yet we have further important steps taken in a forward direction as to the production and treatment of steel by Dr. Siemens and Sir Joseph Whitworth and others, both in this country and abroad.

It is now seventeen years since Mr. Bessemer read a paper at the meeting of the British Association at Cheltenham, which was entitled, "On the Manufacture of Iron and Steel without Fuel."

It is satisfactory to know that Mr. Bessemer has often expressed his firm conviction that had it not been for the publicity given to his invention through the paper which he read before the Mechanical Section of the British Association in 1856, and the great moral support afforded him by men of science whose attention was thereby directed to it, he believes that he would not have succeeded in overcoming the strong opposition with which his invention was met in other quarters.

About this time, or perhaps a little later, a material was produced called "puddled steel," and about the same time the metal known as "homogeneous iron."

The movement which had begun in the production of cheap steel was further assisted and developed by the regenerative furnace of Dr. Siemens, by the introduction of the Siemens-Martin process of making steel, and further and most important progress is suggested by the recent process introduced by Dr. Siemens in making steel direct from the ore.

According to the returns published by the Jury of the International Exhibition of 1852, the total annual produce of steel in Great Britain at that time was 50,000 tons. At the present time there are more than 500,000 tons made by the Bessemer process alone, added to which Messrs. Siemens's works at Landore produce 200,000 tons, besides further quantities which are made by his process at Messrs. Vickers, Messrs. Cammells, the Dowlais, and other works.

I shall not, however, detain you by attempting to trace up the history and progress of steel, nor attempt to notice the various steps by which this branch of industry has been brought to its present important position. My object is to draw attention to this material as to its use and application for *structural and engineering* purposes.

The steel produced by the Bessemer process was at a very early stage employed in rails and wheel-tires. In both these applications the object sought was endurance to resist the effects of wear, and toughness to prevent fracture by blows. There does not exist at present sufficient information to determine accurately the relative values of steel and iron when used for these purposes. As used for wheel-tires, steel had to compete with iron of the highest quality, but it is nevertheless introduced on most of our railways. The iron used in rails was not of such a high quality, and the difference in duration shows a very marked advantage in the employment of steel, the duration of steel rails being variously estimated at from three to six times that of iron.

Steel is also extensively used for ships' plates, and by the War Department for lining the interior of the heaviest guns; while Sir Joseph Whitworth and Messrs. Krupp make guns entirely of steel, though for these purposes the metal is of different quality and differently treated, in order to withstand the enormous concussions to which it is subjected.

And, further, we have steel used in railway-axles, crank-axles for engines, in boilers, in piston-rods, in carriage-springs, and for many other purposes.

But notwithstanding these various employments of steel, there has been, and there continues to be, a difficulty in applying it to engineering structures in this country.

The want of knowledge of the physical properties of steel having been the subject of remark at a discussion at the Institution of Civil Engineers in 1868, a committee, composed of Mr. Fowler, Mr. Scott Russell, Captain Galton, Mr. Berkley, and myself, undertook to conduct a series of experiments upon this subject.

The first were made for the Committee by Mr. Kirkaldy with his testing-machine in London, and were chiefly directed to ascertain the relation which subsists between the resistance of tension, compression, torsion, and transverse strain.

In this series of experiments twenty-nine bars, 15 ft. long, were used, each bar being cut into lengths, and turned or planed into suitable forms for the respective tests, so that a portion of each bar was subjected to each of the above-mentioned tests.

The tensile resistance varied in the different qualities of steel from 28 to 48 tons per inch, and the experiments established conclusively that the relation subsisting between the several resistances of tension, compression, and transverse strain is throughout practically the same as in wrought-iron; that is to say, that a bar of steel whose tensile strength is 50 per cent. above that of wrought-iron will exhibit about the same relative increase of resistance under the other tests.

They further showed that the limit of elasticity in steel is, like that of wrought-iron, rather more than half its ultimate resistance. The total elongation under tensile strain, and the evidences of malleability and toughness, will be referred to hereafter.

The second series recorded in the book published by the Committee gave the results of tempering steel in oil and water. They were made by the officers of the gun-factory at the Royal Arsenal at Woolwich, and show a remarkable increase of strength obtained by this process. This property of steel is now fully recognised and made use of in the steel which forms the lining of the largest guns.

The third series of experiments was made by the Committee upon bars 14 ft. long, $1\frac{1}{2}$ in. in diameter, with the skin upon the metal as it came from the rolls.

The object of these experiments was specially directed to ascertain the *modulus of elasticity*. They were made with the testing-machine at H.M. Dockyard at Woolwich, which machine was placed at our disposal by the Admiralty. The bars were obtained, with some exceptions, in sets of six from each maker, three bars of each set being used in tension and three in compression.

Bars of iron of like dimensions were also tested in the same way, in order to obtain the relative effects in steel and iron. In these experiments sixty-seven steel bars were tested whose tensile strength varied from 32 to 53 tons per inch, and twenty-four iron bars varying from 22 to 29 tons per inch.

The amount of the extensions and compressions were ascertained by *direct measurement*, verniers being for this purpose attached to the bar itself, 10 ft. apart, so that the readings gave the absolute extensions and compressions of this length of the bar.

These experiments, which were very accurately made, showed that the extension and compression of steel per ton per inch was a little less than wrought-iron, that the extension and compression were very nearly equal to each other, and that the modulus of elasticity of steel may be taken at 30,000,000, which result agrees with the conclusions arrived at by American engineers on this subject.

This property of the metal is important in two respects. First, because inasmuch as the extension per ton per inch is practically equal to the compression, it follows that the neutral axis of a structure of steel, strained transversely, will be in the centre of gravity of its section, and that the proper proportion to give to the upper and lower flanges of a girder, when made of the same quality of steel throughout, will be the same as in wrought-iron. Secondly, because the modulus of elasticity of steel is practically equal to that of wrought-iron, and the limit of elasticity is greater, it follows that in a girder of the same proportions as wrought-iron, and strained with an equal proportion of its ultimate tensile strength, the deflection will be greater in the steel than in the iron girder, in the rate of the strength of the metals; so that if it is necessary to make a steel girder for a given span deflect under its load the same amount as an iron girder of the same span, the steel girder must be made of greater depth.

The fourth series of experiments were made by the Committee on riveted steel, and show clearly that the same rules which

apply to the riveting of iron apply equally to steel; that is to say, that the total shearing area of the rivets must be the same, or rather must not be less, than the sectional area of the bar riveted. . . .

We know from established mechanical laws that the limiting spans of structures vary directly as the strength of the material employed in their construction when the proportion of depth to span and all other circumstances remain the same. We know also that, taking an ordinary form of open wrought-iron detached girder (as, for example, when the depth is one-fourteenth of the span), the limiting span in iron, with a strain of 5 tons to the inch upon the metal, is about 600 ft.; and it follows that a steel girder of like proportions, capable of bearing 8 tons to the inch, would have theoretically a limiting span of 960 ft.

This theoretical limiting span of 960 ft. would, however, be reduced by some practical considerations connected with the minimum thickness of metal employed in certain parts, and it would, in effect, become about 900 ft. for a girder of the before-mentioned construction and proportions.

The knowledge of the limiting span of a structure, as has been explained elsewhere, enables us to estimate very quickly, and with close approximation to the truth, the weight of girders required to carry given loads over given spans; and although the limiting spans vary with every form of structure, we can obtain an idea of the effect of introducing steel by the relative weights of steel and iron required in girders of the kind above mentioned.

Assuming a load in addition to the weight of the girder of one ton to the foot, the relative weights under these conditions would be as follows:—

Span.	Weight of steel girder. tons.	Weight of iron girder. tons.
200	57	100
300	150	300
400	320	800

It is not alone in the relative weight or in the relative cost that the advantage of the stronger material is important, but with steel we shall be enabled to cross openings which are absolutely impracticable in iron.

It will naturally be asked why it is that steel is not used in these structures, if such manifest advantages would result from its employment.

The reason is twofold:—

1st. There is a want of confidence as to the reliability of steel in regard to its toughness and its power to resist fracture from sudden strain.

2nd. Steel is produced of various qualities, and we do not possess the means, without elaborate testing, of knowing whether the article presented to us is of the required quality for structural purposes. A third reason, arising probably out of those before mentioned, is found in the fact that in the regulations of the Board of Trade relative to railway structures, although rules are given for the employment of cast-iron and wrought-iron, steel has not, up to the present time, been recognised or provided for.

Now, as regards the question of toughness and malleability, and referring again to Mr. Kirkaldy's experiments, it appears that in the tests of "Bessemer steel" 18 samples were tried under tensile strain, the length of the samples being in round numbers 50 in. and the diameter 1·382 in.; and that when these were subjected to ultimate strain, the elongation at the moment of fracture was in the most brittle example $2\frac{3}{4}$ in., but generally varied from $4\frac{1}{2}$ to $9\frac{1}{2}$ inches.

In the experiments on transverse strain, in which the bars were nearly 2 in. square and only 20 in. between the points of support, all the "Bessemer steel" samples, except two, bent 6 in. without any crack. Again, in the experiments made by the Committee on bars 14 ft. long and $1\frac{1}{2}$ in. in diameter, out of 20 bars of the milder quality of steel, 16 extended more than 8 in., and of these 10 extended more than 12 in. . . .

The treatment by comparison is especially important where metal is required in large masses and of great ductility because the larger the mass and the greater the ductility, the larger and more numerous are the air-cells, and the effect of the pressure is to completely close these cells and render the metal perfectly solid.

By this process mild steel can be made with a strength of 40 tons to the inch, having a degree of ductility equal to that of the best iron.

The more highly carbonised qualities show a decrease of ductility somewhat in the same ratio as the strength increases.

Without going into the numerous achievements of Sir Joseph Whitworth, resulting from the employment of steel, in connection with the extreme accuracy of workmanship produced at his works, or doing more than mention the flat-ended steel shot and shell which pass through iron plates when fired obliquely or penetrate ships' sides below the level of the water, I would call attention to those applications of steel which bear upon its strength and toughness.

In the first place, there are small arms made entirely of steel, of wonderful range and accuracy, capable of penetrating 34 half-inch planks, which is about three times the penetrating power of the Enfield rifle.

Secondly, there are the large guns, also entirely of steel, throwing projectiles from 250 lbs. to 310 lbs. in weight, and burning from 40 to 50 lbs. of powder at a charge, with which a range of nearly $6\frac{1}{4}$ miles is obtained.

In both these cases the degree of strength and toughness required in the metal is much greater than is necessary for engineering structures.

It is unnecessary to occupy more time in multiplying examples of the toughness of steel. It is well known to manufacturers, and must also be well known to many others here present, that steel of the strength of 33 or 36 tons per inch can be made, and is made in large quantities at moderate price, possessing all the toughness and malleability required in engineering structures.

I will proceed, therefore, to the second part of the subject—namely, the want of means of knowing that a given sample of steel is of the quality suited for structural purposes.

With most other metals chemical analysis is in itself a complete and sufficient test of quality, but in steel it is not so. The toughness of steel may be altered by sudden cooling; and although the effect of this operation, and generally the effects of tempering, are greater when the quantity of carbon is considerable, yet it acts more or less in the mild qualities of steel; so that we cannot rely entirely on the aid of the chemist, but must fall back on mechanical tests. And in point of fact, seeing that the qualities required are mechanical, it is no more than reasonable that the test should be mechanical; for this includes not only the test of material but of workmanship.

Now there are two descriptions of mechanical testing, which may be distinguished as destructive and non-destructive—the one being beyond and the other within the elastic limit of the material. The destructive test is that usually applied to a part of an article manufactured, as, for example, a piece cut off a boiler plate and tested by absolute rupture, or by bending or otherwise, whereby the strength and quality of the material in the plate is known.

The non-destructive test is that usually applied to the finished work, as in the test of a boiler by hydraulic pressure, or the testing of a gun by the proof-charge. The strain in this case is made greater than that which will arise in the daily use of the article, but is not so greatly in excess as to be beyond the elastic limit of the material.

As regards engineering structures, this second test is easy of application; but it affords no sufficient criterion that the metal possesses that degree of toughness necessary to resist the action of sudden strains.

It may be said that engineers may ascertain for themselves, by inspection and testing at the works, that they are being supplied with the material that they require; but assuming that the tests and mode of testing were in all respects satisfactory to them, and that the metal supplied was of the right quality, we have still to comply with the conditions of the Act for the Regulation of Railways, and we must satisfy the Government Inspector.

It is not to be supposed that he can attend all the required tests at the works; and the question remains, how is the Inspecting Officer of the Board of Trade to be enabled to distinguish the quality of metal in a finished bridge, when he is called upon to give a certificate that it is safe for public traffic?

If we could adduce clear and distinct evidence that the metal used for a bridge was of a quality which would bear 8 tons to the inch with as much safety as common iron can bear 5 tons, there can be no reasonable doubt that the Board of Trade would make suitable provision in its regulations for the employment of such material.

The difficulty lies in the want of something whereby the quality of the metal may be known and relied upon with confidence by others besides those who made the article.

In gold and silver this is accomplished by the stamp put upon

them, in guns and small arms we have the proof-mark, but in iron and steel we have nothing whereby the one quality of metal can be distinguished from another; and until some sufficient means be devised for this purpose, it is difficult to see how we are to escape from the position in which we are now placed—namely, that while we possess a material by which we can increase considerably the spans and diminish the weight and cost of engineering works, we are restricted to make designs and construct our works by a rule made for wrought iron, and adapted to the lowest quality of that material.

As the rule made by the Board of Trade in respect of wrought-iron railway structures may not be generally known, I here give it :—

"In a wrought-iron bridge the greatest load which can be brought upon it, added to the weight of the superstructure, should not produce a greater strain on any part of the material than 5 tons per inch."

It will be observed that this 5 tons per inch is the governing element, irrespective entirely of the quality of metal used; and it is obvious that a rule so framed must act as a discouragement to any endeavour to improve the quality of metal, while it tends to induce the employment of the cheapest and most inferior descriptions which can be made under the name of wrought-iron.

In endeavouring to seek an amendment of the rules, which will permit of the employment of steel or other metal of higher strength than 5 tons to the inch, I feel bound to say that I do not consider that the Board of Trade is alone responsible for the position in which the question now stands; and as regards the Government Inspecting Officers, I can only say that in the numerous transactions I have had with them, and although differences of opinion have occasionally arisen, yet, considering the responsibility which rests upon them, I have found them anxious to afford all reasonable facilities so far as their instructions permitted.

The first step to be taken is to put our testing on a systematic and satisfactory basis.

The second is to establish some means whereby metal which has been tested can have its quality indicated upon it in such manner that it can be practically relied upon.

The experiments before referred to establish, sufficiently for all practical purposes, that the relation or proportion between the resistances to tension, compression, torsion, and transverse strain, is about the same in steel as in wrought-iron.

The testing required is therefore reduced to that necessary for ascertaining two properties, namely the strength and the toughness or ductility.

The strength may be readily ascertained, and no difficulty arises on that head.

The whole question turns upon the test for ductility, or the resistance to fracture by blows or sudden strain; and it must be admitted that the tests employed for this purpose are not framed on any regular or satisfactory basis.

Without, however, attempting to say what description of test may be found the best for ascertaining the property of ductility, it may be observed that what is required for this test is a definite basis to act upon, and that the samples should be so made as to render the test cheap, expeditious, and easy of application.

The next requirement is that when a piece of metal has been tested, and its qualities of strength and toughness ascertained, there should be some means of denoting its quality in an authentic manner.

To a certain extent this is already done in iron by the mark of the maker; but something more than this is necessary to fulfil the required conditions in steel.

What is termed steel, is iron with a small proportion of carbon in it. These two ingredients are necessary to constitute steel; and there may or may not be present in very small quantities graphite, silicon, manganese, sulphur, and phosphorus.

In connection with the experiments made by the Committee, fourteen of the samples were tested by Mr. E. Richards, of the Barrow Steel Works, five of which were kindly repeated by Dr. Odling.

Although there are some discrepancies in the results which we cannot account for, yet some of the characteristics are brought out clearly.

It appears that manganese may be present to the extent of four-tenths per cent. without injury either to the strength or ductility, but sulphur and phosphorus, except in extremely small quantities, are fatal to ductility.

In the samples tried by the Committee and Mr. Kirkaldy, the quantity of carbon varied from $\frac{1}{4}$ per cent. to nearly 1 per cent.; yet with this small variation in the carbon the strength ranged from thirty-three tons to nearly fifty-three tons per in.: and the ductility, represented by the ratio which the fractured area bore to the original section of the bar, varied from five-tenths in the tough qualities, until in the harder samples there was no diminution perceptible.

All these materials are called steel, and have the same external appearance; but possessing, as they do, such a range of strength and such a variation in ductility, it becomes absolutely essential that there should be some classification or means of knowing the respective qualities among them.

The want of such classification casts an air of uncertainty over the whole question of steel, and impedes its application. To this want of knowledge is to be ascribed the circumstance that many professional men regard the material as altogether unreliable; while large consumers of steel, in consequence of the uncertainty of the quality they buy in the market, seek to establish works on their own premises and make their own steel.

I ought, I know, to apologise for detaining you so long on this one question of steel, but I consider that the difficulties under which it is placed are affecting interests of considerable importance.

Not only is a large and useful field for the employment of steel practically closed, but the progress of improvement in engineering structures is impeded both in this country and in other parts of the world where English engineers are engaged.

For in consequence of the impediments to its employment in England, very few English engineers turn their attention to the use of steel. They are accustomed to make their designs for iron, and when engaged in works abroad where the Board of Trade rules do not apply, they continue for the most part to send out the old-fashioned ponderous girders of common iron, in cases where the freight and difficulties of carriage make it extremely desirable that structures of less weight and more easy transport should be employed.

In conclusion, and while thanking you for the patience with which you have heard me on this subject, I would observe that we possess in steel a material which has been proved, by the numerous uses to which it is applied, to be of great capability and value: we know that it is used for structural purposes in other countries, as, for example, in the Illinois and St. Louis Bridge in America, a bridge of three arches, each 500 ft. span; yet in this country, where "modern steel" has originated and has been brought to its present state of perfection, we are obstructed by some deficiency in our arrangements, and by the absence of suitable regulations by the Board of Trade, from making use of it in engineering works.

And I have considered it right to draw your attention to the position in which this question stands, well knowing that I could not address any body of gentlemen more capable of improving and systematising our methods of testing, or better able to devise effectual means for removing the impediments to the use of steel, than are to be found in the scientific and practical men who form the Mechanical Section of the British Association.

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